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ART. XXXIII.—*Address of Dr. John L. LeConte, the retiring President of the American Association for the Advancement of Science, at the meeting in August*, 1875, *at Detroit.*

THE founders of science in America, and the other great students of nature, who have in previous years occupied the elevated position in which I now stand, have addressed you upon many momentous subjects. In fulfilling the final duty assigned to your presidents by the laws of the Association, some have spoken to you in solemn and wise words concerning the duties and privileges of men of science, and the converse duties of the nation toward those earnest and disinterested promoters of knowledge. Others again have given you the history of the developement of their respective branches of study, and their present condition, and have, in eloquent diction, commended to your gratitude those who have established on a firm foundation the basis of our modern systems of investigation.

The recent changes in our constitution, by which you are led to expect from your two vice-presidents, and from the chairman of the Chemical Sub-section, addresses on the progress made during the past year, restrain me from invading their peculiar fields of labor, by alluding to scientific work which has been accomplished since our last meeting. While delicacy forbids me from so doing, I am equally debarred from repeating to you the brief sketch I endeavored to give at a former meeting *

* Proceedings Am. Assoc. Adv. Sci., xxi, Portland.

of the history, and present condition of Entomology in the United States.

But it has appeared to me that a few thoughts, which have impressed themselves upon my mind, touching the future results to be obtained from certain classes of facts not yet fully developed on account of the great labor required for their proper comparison, may not be without value. Even if the facts be not new to you, I hope to be able, with your kind attention, to present them in such way as to be suggestive of the work yet to be done.

It has been perhaps said, or at least it has been often thought, that the first mention of the doctrine of evolution, as now admitted to a greater or less degree by every thinking man, is found in Ecclesiastes, i, 9: "The thing that hath been is that which shall be; and that which is done is that which shall be done; and there is no new thing under the sun. Is there anything whereof it may be said, see, this is new? It hath been already of old time, which was before us."

Other references to evolutionary views in one form or another occur in the writings of several philosophers of classic times, as you have had recent cause to remember.

Whether these are to be considered as an expression of a perfect truth in the very imperfect language which was alone intelligible to the nation to whom this sacred book was immediately addressed on the one hand: and the happy guesses of philosophers, who by deep intuition had placed themselves in close sympathy with the material universe on the other hand, I shall not stop to inquire. The discussion would be profitless, for modern science in no way depends for its magnificent triumphs of fact and thought upon any utterances of the ancients. It is the creation of patient, intelligent labor of the last two centuries, and its results can be neither confuted nor confirmed by anything that was said, thought or done, at an earlier period. I have merely referred to these indications of doctrines of evolution to recall to your minds that the two great schools of thought, which now divide philosophers, have existed from very remote times. They are, therefore, in their origin, probably independent of correct scientific knowledge

You have learned from the geologists, and mostly from those of the present century, that the strata of the earth have been successively formed from fragments more or less comminuted by mechanical action, more or less altered by chemical combination and molecular rearrangements. These fragments were derived from strata previously deposited, or from material brought up from below, or even thrown down from above, or from the debris of organic beings which extracted their mineral constituents from surrounding media. Nothing new has been added, everything is old; only the arrangement of the parts is

new, but in this arrangement definite and recognizable unchanged fragments of the old frequently remain. Geological observation is now so extended and accurate that an experienced student can tell from what formation, and even from what particular locality, these fragments have been derived.

I wish to show that this same process has taken place in the organic world, and that by proper methods we can discover in our fauna and flora the remnants of the inhabitants of former geologic times, which remain unchanged, and have escaped those influences of variation which are supposed to account for the differences in organic beings of different periods.

Should I succeed in this effort we will be hereafter enabled in groups of animals which are rarely preserved in fossil condition to reconstruct, in some measure, the otherwise extinct faunæ, and thus to have a better idea of the sequence of generic forms in time. We will also have confirmatory evidence of certain changes which have taken place in the outline of the land and the sea. More important still, we will have some indications of the time when greater changes have occurred, the rock evidence of which is now buried at the bottom of the ocean, or perhaps entirely distroyed by erosion and separation. Of these changes, which involved connections of masses of lands, no surmise could be made except through evidence to be gained in the manner of which I am about to speak.

My illustrations will naturally be drawn from that branch of zoology with which I am most familiar; and it is indeed to your too partial estimate of my studies in that science that I owe the privilege of addressing you on the present occasion.

There are, as you know, a particular set of Coleoptera which affect the seashore; they are not very numerous at any locality, but among them are genera which are represented in almost every country of the globe. Such genera are called cosmopolitan, in distinction to those which are found only in particular districts. Several of these genera contain species which are very nearly allied, or sometimes, in fact, undistinguishable and therefore identical, along extended lines of coasts.

Now it happens that some of these species, though they never stray from the ocean shore inland, are capable of living upon similar beaches on fresh water lakes, and a few are found in localities which are now quite inland.

To take an example, or rather several examples together, for the force of the illustration will be thereby greatly increased.

Along the whole of the Atlantic, and the greater part of the Pacific coast of the United States, is found in great abundance on sand beaches, a species of Tiger-beetle, *Cicindela hirticollis*, an active, winged, and highly predaceous insect; the same species occurs on the sand beaches of the great lakes, and were it con-

fined to these and similar localities, we would be justified in considering it as living there in consequence solely of the resemblance in the conditions of its existence. But it is also found, though in much less abundance, in the now elevated region midway between the Mississippi and Rocky Mountains. Now, this is the part of the continent which, after the division of the great intercontinental gulf in Cretaceous times, finally emerged from the bed of the sea, and was in the early and middle Tertiary converted into a series of fresh water lakes. As this insect does not occur in the territory extending from the Atlantic to beyond the western boundary of Missouri, nor in the interior of Oregon and California, I think that we should infer that it is an unchanged survivor of the species which lived on the shores of the Cretaceous ocean, when the intercontinental gulf was still open, and a passage existed, moreover, toward the southwest, which connected with the Pacific.

The example I have given you of the geographical distribution of *Cicindela hirticollis* would be of small value were it an isolated case; nor would I have thought it worthy of occupying your time on an occasion like this, which is justly regarded as one for the communication of important truths. This insect, which I have selected as a type for illustrating the methods of investigation to which I invite your attention, is, however, accompanied more or less closely by other Coleoptera, which like itself, are not particular as to the nature of their food so long as it be other living insects, and apparently are equally indifferent to the presence of large bodies of salt water. First, there is *Cicindela lepida*, first collected by my father near Trenton, N. J., afterward found on Coney Island, near New York, and received by me from Kansas and Wisconsin; not, however, found west of the Rocky Mountains. This species, thus occurring in isolated and distant localities, is probably in progress of extinction, and may or may not be older than *Cicindela hirticollis*. I am disposed to believe, as no representative species occurs on the Pacific coast, and from its peculiar distribution, that it is older. Second, there is *Dyschirius pallipennis*, a small Carabide, remarkable among the other species of the genus by the pale wing covers, usually ornamented with a dark spot. This insect is abundant on the Atlantic coast from New York to Virginia, unchanged in the interior parts of the Mississippi Valley, represented at Atlantic City, New Jersey, by a large and quite distinct specific form, *D. sellatus*, and on the Pacific coast by two or three species of larger size and different shape, which, in my less experienced youth, I was disposed to regard as a separate genus, *Akephorus*. This form is, therefore, in a condition of evolution - how, I know not—our descendants may. The Atlantic species are

winged, the Pacific one, like a large number of insects of that region, are without wings.

Accompanying these are Coleoptera of other families, which have been less carefully studied, but I will not trespass upon your patience by mentioning more than two. *Bledius pallipennis* (*Staphylinidæ*) is found on salt marshes near New York, on the Southern sea-coast and in Kansas—*Ammodonus fossor*, a wingless Tenebrionide, Trenton, sea-shore near New York, and Valley of the Mississippi at St. Louis; thus nearly approximating *Cicindela lepida* in distribution.

We can thus obtain by a careful observation of the localities of insects, especially such as affect sea-shore or marsh, and those which being deprived of their favorite surroundings have shown, if I may so express myself, a patriotic clinging to their native soil, most valuable indications in regard to the time at which their unmodified ancestors first appeared upon the earth. For it is obvious that no tendency to change in different directions by "numerous successive slight modifications"* would produce a uniform result in such distant localities, and under such varied conditions of life. Properly studied, these indications are quite as certain as though we found the well preserved remains of these ancestors in the mud and sand strata upon which they flitted or dug in quest of food.

Other illustrations of survivals from indefinitely more remote times I will give you, from the Coleopterous fauna of our own country, though passing time admonishes me to restrict their number.

To make my remarks intelligible, I must begin by saying that there are three great divisions of Coleoptera, which I will name in the order of their complication of structural plan: 1. Rhynchophora; 2. Heteromera; 3. Ordinary or normal Coleoptera; the last two being more nearly allied to each other than either is to the first. I have in other places exposed the characters of these divisions, and will not detain you by repeating them.

From paleontological evidence derived from other branches of zoology we have a right to suppose, if this classification be correct, that these great types have been introduced upon the earth in the order in which I have named them.

Now, it is precisely in the first and second series that the most anomalous instances of geographical distribution occur; that is to say, the same or nearly identical genera are represented by species in very widely separated regions, without occurring in intermediate or contiguous regions. Thus there is a genus *Emeax*, founded by Mr. Pascoe, upon an Australian species, which, when I saw it, I recognized as belonging to

* Origin of Species, 1869, 227.

Nyctoporis, a California genus, established many years before, and in fact barely specifically distinct from *N. galeata*. Two other examples are *Othnius* and *Eupleurida*, United States genera, which are respectively equivalent to *Elacatis* and *Ischalia*, found in Borneo. Our native genera, *Eurygenius* and *Toposcopus*, are represented by scarcely different forms in Australia. All these belong to the second series (*Heteromera*), and the number of examples might be greatly increased with less labor on my part than patience on yours.

A single example from the Rhynocophora, and I will pass to another subject.

On the sea-coast of California, extending to Alaska, is a very anomalous insect whose affinities are difficult to discern, called *Emphyastes fucicola*, from its occurrence under the sea-weed cast up by the waves. It is represented in Australia by several species of a nearly allied genus *Aphela*, found in similar situations.

In all entomological investigations relating to geographical distribution we are greatly embarrassed by the multitude of species, and by the vague and opinionative genera founded upon characters of small importance. The Coleoptera alone, thus far described, amount to over 60,000 so-called species, and there are from 80,000 to 100,000 in collections. Under these circumstances it is quite impossible for one person to command either the time or the material to master the whole subject, and, from the laudable zeal of collectors to make known what they suppose to be new objects, an immense amount of synonymy must result. Thus, in the great Catalogus Coleopterorum of Gemminger and Harold, a permanent record of the untiring industry of those two excellent entomologists, species of the genus *Trechicus*, founded by me upon a small North American insect, are mentioned under five generic names, only one of which is recognized as a synonym of another. These generic headings appear in so remote pages of the volume as 135, 146 and 289.

The two closely allied genera of Rhynchophora mentioned above are separated by no less than 168 pages. It is, therefore, plain that, before much progress can be made in the line of research which I have proposed to you, whereby we may recover important fragments of the past history of the earth, entomology must be studied in a somewhat different manner from that now adopted. The necessity is everyday more apparent that descriptions of heterogeneous material are rather obstructive than beneficial to science, except in the case of extraordinary forms likely to give information concerning geographical distribution or classification. Large typical collections, affording abundant material for comparison, for the

approximation of allied forms, and the elimination of doubtful ones, must be accumulated; and in the case of such perishable objects as those we are now dealing with, must be placed where they can have the protecting influences both of climate and personal care.

At the same time, for this investigation the study of insects is peculiarly suitable: not only on account of the small size, ease of collecting, and little cost of preserving, the specimens, but because, from their varied mode of life in different stages of development, and perhaps for other reasons, the species are less likely to be destroyed in the progress of geological changes. (For a fuller discussion of these causes and of several other subjects which are briefly mentioned in this address, the reader may consult an excellent memoir by my learned friend, Mr. Andrew Murray, "On the Geographical Relations of the Chief Coleopterous Faunæ," Journal of Linnæan Society, Zoology, vol. xi.) Cataclysms and submergences, which would annihilate the higher animals, would only float the temporarily asphyxiated insect, or the tree trunks containing the larvæ and pupæ to other neighboring lands. However that may be, I have given you some grounds for believing that many of the species of insects now living existed in the same form before the appearance of any living genera of mammals, and we may suppose that their unchanged descendants will probably survive the present mammalian fauna, including our own race.

I may add, moreover, that some groups, especially in the Rhynchophora, which, as I have said above, I believe to be the earliest introduced of the Coleoptera, exhibit with compact and definite limits, and clearly defined specific characters, so many generic modifications that I am compelled to think that we have in them an example of long sought unbroken series extending, in this instance, from early Mesozoic to the present time, and of which very few forms have become extinct.

I have used the word species so often that you will doubtless be inclined to ask, what, then, is understood by a species? Alas! I can tell you no more than has been told recently by many others. It is an assemblage of individuals, which differ from each other by very small or trifling and inconstant characters, of much less value than those in which they differ from any other assemblage of individuals. Who determines the value of these characters? The experienced student of that department to which the objects belong. Species are, therefore, those groups of individuals representing organic forms which are recognized as such by those who from natural power and education are best qualified to judge.

You perceive, therefore, that we are here dealing with an

entirely different kind of information from that which we gain from the physical sciences; everything there depends on accurate observation, with strict logical consequences derived therefrom. Here the basis of our knowledge depends equally on accurate and trained observation, but the logic is not formal but perceptive.

This has been already thoroughly recognized by Huxley* and Helmholtz,† and others, but we may properly extend the inquiry into the nature and powers of this æsthetic perception somewhat further. For it is to this fundamental difference between biological and physical sciences that I will especially invite your attention.

Sir John Lubbock,‡ quoting from Oldfield,§ mentions that certain Australians "were quite unable to realize the most vivid artistic representations. On being shown a picture of one of themselves, one said it was a ship, another a kangaroo, not one in a dozen identifying the portrait as having any connection with himself.

These human beings, therefore, with brains very similar to our own, and, as is held by some persons, potentially capable of similar cultivation with ourselves, were unable to recognize the outlines of even such familiar objects as the features of their own race. Was there any fault in the drawing of the artist? Probably not. Or in the eye of the savage? Certainly not, for that is an optical instrument of tolerably simple structure, which cannot fail to form on the retina an accurate image of the object to which it is directed. Where then is the error? It is the want of capacity of the brain of the individual (or rather of the race in this instance) to appreciate the resemblance between the outline, the relief, the light and shade of the object pictured, and the flat representation in color: in other words, a want of "artistic tact" or æsthetic perception.

A higher example of a similar phenomenon I have myself seen; many of you, too, have witnessed it, for it is of daily occurrence. It is when travelers in Italy, having penetrated to the temple of Art, even the hall of the Tribune at Florence, stand in presence of the most perfect works of art which it has

* "A species is the smallest group to which distinctive and invariable characters can be assigned."—*Principles and Methods of Palæontology, Smithsonian Report*, 1869, 378.

† "I do not mean to deny that in many branches of these sciences, an intuitive perception of analogies and a certain artistic tact play a conspicuous part. In natural history * * * it is left entirely to this tact, without a clearly definable rule, to determine what characteristics of species are important or unimportant for purposes of classification, and what divisions of the animal or vegetable kingdom are more natural than others."—*Relation of the Physical Sciences to Science in General; Smiths. Report*, 1871, 277.

‡ Prehistoric Times, p. 440.

§ On the Aborigines of Australia, Trans. Ethnological Soc., New series, vol. iii.

been given to man to produce, and gaze upon them with the same difference that they would show to the conceptions of mediocre artists exhibited in our shops. Perhaps they would even wonder what one can find to admire in the unrivalled collection which is there assembled. There is surely wanting in the minds of such persons that high, æsthetic sense, which enables others to enter into spiritual harmony with the great artists whose creations are before them. Creations I said, and I use the word intentionally. If there is one power of the human soul, which more nearly than any other approaches the faculty of creation, it is that which the almost inspired artist develops out of a rude block of stone, or out of such mean materials as canvas and metallic pastes of various colors, figures which surpass in beauty and in power of exciting emotion the objects they profess to represent.

Yet these unæsthetic and nonappreciative persons are just as highly educated, and in their respective positions as good and useful members of the social organism, as any that may be found. I maintain only, they would never make good students of biology.

In like manner, by way of illustrating the foregoing observations, there are some, who, in looking at the phenomena of the external universe, may recognize only chance, or the "fortuitous concourse of atoms," producing certain resultant motions. Others, having studied more deeply the nature of things, will perceive the existence of laws, binding and correlating the events they observe. Others again, not superior to the latter in intelligence, nor in power of investigation, may discern a deeper relation between these phenomena, and the indications of an intellectual or æsthetic or moral plan, similar to that which influences their own actions, when directed to the attaining of a particular result.

These last will recognize in the operations of nature the direction of a human intelligence, greatly enlarged, capable of modifying at its will influences beyond our control; or they will appreciate in themselves a resemblance to a superhuman intelligence which enables them to be in sympathy with its actions. Either may be true in individual instances of this class of minds; one or the other must be true; I care not which, for to me the propositions are in this argument identical, though in speculative discussions they may be regarded as at almost the opposite poles of religious belief. All that I plead for is this, that those who have not this perceptive power, and who in the present condition of scientific discussion are numerically influential, will have tolerance for those who possess it; and that the idea of the latter may not be entirely relegated to the domain of superstition and enthusiasm.

In the case of want of perception of the Australian, a very simple test can be applied. It is only to photograph the object represented by the artist, and compare the outlines and shades of the photograph with those of the picture. If they accord within reasonable limits, the picture is correct to that extent; at least, however bad the artist, the human face could never be confounded with a ship or a kangaroo.

Can we apply a similar test to the works of nature? I think we can. Suppose that man—I purposely use the singular noun to indicate that all human beings of similar intelligence and education working toward a definite end, will work in a somewhat similar manner—suppose, then, I say, that man, endeavoring to carry out some object of importance, devises a method of so doing, and creates for that purpose a series of small objects, and we find that these small objects naturally divide and distribute themselves in age and locality, in a similar manner to that in which the species of a group of organisms are divided in space, and distributed in time; and that the results of man's labor are thus divided and distributed on account of the necessary inherent qualities of his intelligence and methods of action, is not the resemblance between human reason and the greater powers which control the manifestations of organic nature apparent?

I now simply present to you this investigation. Time is wanting for me to illustrate it by even a single example, but I feel sure that I have in the minds of some of you already suggested several applications of it to the principle I wish to teach—the resemblance in the distribution of the works of nature to that of human contrivances evolved for definite purposes.

If this kind of reasoning commends itself to you, and you thus perceive resemblances in the actions of the Ruler of the Universe to those of our own race, when prompted by the best and highest intellectual motives, you will be willing to accept the declaration of the ancient text, "He doeth not evil, and abideth not with the evil inclined. Whatever he hath done is good";* or that from our own canon of Scripture, "With him is wisdom and strength, he hath counsel and understanding.†

The æsthetic character of natural history, therefore, prevents the results of its cultivation from being worked out with the precision of a logical machine, such as with correct data of observation and calculation would be quite sufficient to formulate the conclusions of physical investigation. According as the perception of the relations of organic beings among themselves becomes more and more enlarged, the interpretation of these relations will vary within limits; but we will be continually approximating higher mental or spiritual truth.

* Desatir, p. 2. † Job, xii, 13.

This kind of truth can never be revealed to us by the study of inorganic aggregations of the universe. The molar, molecular and polar forces, by which they are formed, may be expressed, so far as science has reduced them to order, by a small number of simply formulated laws, indicative neither of purpose nor intelligence, when confined within inorganic limits. In fact, taking also the organic world into consideration, we as yet see no reason why the number of chemical elements known to us should be so large as it is, and go on increasing almost yearly with more minute investigation. To all appearance the mechanical and vital structure of the universe would remain unchanged if half of them were struck out of existence.

Neither is there any evidence of intelligence or design in the fact that the side of the moon visible to us exhibits only a mass of volcanoes. Yet upon the earth, without the volcano and the earthquake and the elevating force of which they are the feeble indications, there would be no permanent separation of land and water; consequently no progress in animal and vegetable life beyond what is possible in the ocean. To us, then, as sentient beings, the volcano and the earthquake, viewed from a biological standpoint, have a profound significance.

It is indeed difficult to see in what manner the student of purely physical science is brought to a knowledge of any evidences of intelligence in the arrangement of the universe. The poet, inspired by meditating on the immeasurable abyss of space, and the transcendent glories of the celestial orbs, has declared,

"The undevout astronomer is mad."

And his saying had a certain amount of speciousness, on account of the magnitude of the bodies and distances with which the student of the stars is concerned. This favorite line is, however, only an example of what an excellent writer has termed "the unconscious action of volition upon credence," and it is properly in the correlations of the inorganic world that we may hope to exhibit, with clearness, the adaptations of plan prefigured and design executed.

In the methods and results of investigation, the mathematician differs from both the physicist and the biologist. Unconfined, like the former, by the few simple relations by which movements in the inorganic world are controlled, he may not only vary the form of his analysis, almost at pleasure, making it more or less transcendental in many directions, but he may introduce factors or relations, apparently inconceivable in real existence, and then interpret them into results quite as real as those of the legitimate calculus with which he is working, but lying outside of its domain.

If biology can ever be developed in such manner that its results may be expressed in mathematical formulæ, it will be the pleasing task of the future analyst to ascertain the nature of the inconceivable (or imaginary as they are termed in mathematics) quantities which must be introduced when changes of form or structure take place. Such will be analytical morphology, in its proper sense; but it is a science of the future, and will require for its calculus a very complex algebra.

In the observation of the habits of interior animals we recognize many complications of action, which, though directed to the accomplishment of definite purposes, we do not entirely comprehend. They are, in many instances, not the result of either the experience of the individual, or the education of its parents, who, in low forms of animals, frequently die before the hatching of the offspring. These actions have been grouped together, whether simple or complex, as directed by what we are pleased to call instinct, as opposed to reason. Yet there is every gradation between the two.

Among the various races of dogs, the companions of man for unnumbered centuries, we observe not only reasoning powers of a rather high order, but also distinct traces of moral sentiments similar to those possessed by our own race. I will give no examples, for many may be found in books with which you are familiar. Actions evincing the same mental attributes are also noticed in wild animals which have been tamed. You will reply that these qualities have been developed by human education; but not so, there must have been a latent capacity in the brain to receive the education and to manifest the results by the modification of the habits. Now it is because we are vertebrates, and the animals of which I have spoken are vertebrates, that we understand, though imperfectly, their mental processes, and can develop the powers that are otherwise latent. Could we comprehend them more fully we would find, and we do find from time to time in the progress of our inquiries, that what was classed with instinct is really intellection.

When we attempt to observe animals belonging to another sub-kingdom, Articulata, for instance, such cases as bees, ants, termites, etc., which are built upon a totally different plan of structure, having no organ in common with ourselves, the difficulty of interpreting their intellectual processes, if they perform any, is still greater. The purposes of their actions we can divine only by their results. But anything more exact than their knowledge of the objects within their scope, more ingenious than their methods of using those objects, more complex yet well devised than their social and political systems, it is impossible to conceive.

We are not warranted in assuming that these actions are instinctive, which, if performed by a vertebrate, we would call rational. Instead of concealing our ignorance under a word which thus used comes to mean nothing, let us rather admit the existence here of a rational power, not only inferior to ours, but also different.

Thus, proceeding from the highest forms in each type of animal life to the lower, and even down to the lowest, we may be prepared to advance the thesis that all animals are intelligent in proportion to the ability of their organization to manifest intelligence to us, or to each other; that wherever there is voluntary motion there is intelligence, obscure, it may be, not comprehended by us, but comprehended by the companions of the same low grade of structure.

However this may be, I do not intend to discuss the subject at present, but only wish, in connection with this train of thought, to offer two suggestions.

The first is that, by pursuing different courses of investigation in biology, we may be led to opposite results. Commencing with the simplest forms of animal life, or with the embryo of the higher animals, it may be very difficult to say at what point intelligence begins to manifest itself; our attention is concentrated, therefore, upon those functions which appear to be the result of purely mechanical arrangements, acted upon by external stimuli. The animal becomes to our perception an automaton, and, in fact, by exercising some of the nervous organs last developed in its growth, we can render an adult animal an automaton, capable of performing only those habitual actions to which its brain, when in perfect condition, had educated the muscles of voluntary motion. On the other hand, commencing with the highest group in each type, and going downward, either in structural complication, or in age of individual, it is impossible to fix the limit at which intelligence ceases to be apparent.

I have in this subject, as in that of tracing the past history of our insects, in the first part of this address, preferred the latter mode of investigation: taking those things which are nearest to us in time or structure, as a basis for the study of those more remote.

The second consideration is, since it is so difficult for us to understand the mental processes, whether rational or instinctive (I care not by what name they are called) of beings more or less similar, but inferior to ourselves, we should exercise great caution when we have occasion to speak of the designs of One who is infinitely greater. Let us give no place to the crude speculations of would-be teleologists, who are, indeed, in great part refuted already by the progress of science, which

continually exhibits to us higher and more beautiful relations between the phenomenon of nature "than it hath entered into the mind of man to conceive." Let not our vanity lead us to believe that because God has deigned to guide our steps a few paces on the road to truth, we are justified in speaking as if He had taken us into intimate companionship, and informed us of all His counsels.

If I have exposed my views on these subjects to you in an acceptable manner, you will perceive that in minds capable of receiving such impressions biology can indicate the existence of a creative or directive power, possessing attributes, some of which resemble our own, and controlling operations which we may feebly comprehend. Thus far natural theology, and no farther.

What then is the strict relation of natural history or biology to that great mass of learning and influence which is commonly called theology; and to that smaller mass of belief and action which is called religion?

Some express the relation very briefly, by saying that science and religion are opposed to each other. Others again that they have nothing in common. These expressions are true of certain classes of minds; but the greater number of thinking and educated persons see that though the ultimate truths taught by each are of quite distinct nature, and can by no means come in conflict, inasmuch as they have no point in common, yet so far as these truths are embodied in human language and manipulated by human interests, they have a common dominion over the soul of man. According to the method of their government, they may then come into collision, even as the temporal and spiritual sovereigns of Japan occasionally did, before the recent changes in that country.

In answering the query above proposed, it will be necessary to separate the essential truths of religion from the accessories of tradition, usage, and most of all, organizations and interpretations, which have, in the lapse of time, gathered around the primitive or revealed truth.

With the latter the scientific man must deal exactly like other men; he must take it, or reject it, according to his spiritual gifts, but he must not, whatever be his personal views, discuss it or assail it as a man of science, for within his domain of investigation it does not belong.

With regard to the accessories of traditions, interpretations, etc., our answer may be clearer when we have briefly reviewed some recent events in what has been written about as the Conflict of Religion and Science. Some centuries ago, great theological disgust was produced by the announcement that the sun and not the earth was the center of the planetary system.

A few decades ago profound dissatisfaction was shown that the evidence of organic life on the planet was very ancient. Recently some annoyance has been exhibited because human remains have been found in situations where they ought not to have been, according to popularly received interpretation: and yet more recently much apprehension has been felt at the possible derivation of man from some inferior organism; an hypothesis framed simply because in the present condition of intellectual advancement no other can be suggested.

Yet all these facts. but the last, which still is an opinion, have been accepted, after more or less bitter controversy on both sides, and the fountain of spiritual truth remains unclouded and undiminished. New interpretations for the sacred texts, supposed to be in conflict with the scientific facts, have been sought and found without difficulty. These much feared facts have, moreover, given some of the strongest and most convincing illustrations to modern exhortation and religious instruction.

Thus, then, we see that the influence of science upon religion has been beneficial. Scholastic interpretations, founded upon imperfect knowledge, or no knowledge, but mere guess, have been replaced by sound criticisms of the texts, and their exegesis in accordance with the times and circumstances for which they were written.

It must be conceded by fair minded men of both sides that these controversies were carried on at times with a rudeness of expression and bitterness of feeling now abhorrent to our usages. The intellectual wars of those days partook of the brutality of physical war, and the horrors of the latter, as you know, have been ameliorated only within a very few years.

I fear that the unhappy spirit of contention still survives, and that there are yet a few who fight for victory rather than for truth. The deceptive spirit of Voltaire still buds forth occasionally; he, who, as you remember, disputed the organic nature of fossil shells, because in those days of schoolmen their occurrence on mountains would be used by others as a proof of a universal Noachian deluge. The power of such spirits is fortunately gone for any potent influence for evil, gone with the equally obstructive influence of the scholastics with whom they formerly contended.

Since then there is no occasion for strict science and pure religion to be in conflict, how shall the peace be kept between them? By toleration and patience. Toleration toward those who believe less than we do, in the hope that they, by cultivation or inheritance of æsthetic perception, will be prepared to accept something more than matter and energy in the universe, and to believe that vitality is not altogether undirected colloid chemistry.

Toleration also toward those who, on what we think misunderstood or insufficient evidence demand more than we are prepared to admit, in the hope that they will revise additional texts which seem to conflict, or may hereafter conflict, with facts deduced from actual study of nature, and thus prepare their minds for the reception of such truths as may be discovered, without embittered discussions.

Patience, too, must be counseled, for much delay will ensue before this desired result is arrived at; patience under attack, patience under misrepresentation, but never controversy.

Thus will be hastened the time when the glorious, all-sufficient spiritual light, which though given through another race, we have adopted as our own, shall shine with its pristine purity, freed from the incrustations with which it has been obscured by the vanity of partial knowledge and the temporary contrivances of human polity.

So, too, by freely extended scientific culture, may we hope that the infinitely thicker and grosser superstitions and corruptions will be removed, which greater age and more despotic governments have accumulated around the less brilliant though important religions of our Asiatic Aryan relatives. These accretions being destroyed, the principal difficulty to the reception by those nations of higher spiritual truths will be obviated, and the intelligent Hindoo or Persian will not be tardy in recognizing, in the pure life and elevated doctrine of the sincere Christian, an addition to and fuller expression of religious precepts with which he is familiar. In this manner alone may be realized the hope of the philosopher, the dream of the poet, and the expectation of the theologian—a universal science, and a universal religion, co-operating harmoniously for the perfection of man and the glory of his Creator.

ART. XXXIV.—*On the Temperature attainable by Rock-crushing, and its Consequences*;* by ROBERT MALLET, F.R.S.

IN developing the theory of volcanic heat and energy embraced in his paper "On the Nature and Origin of Volcanic Heat and Energy" (Phil. Trans., Part I, 1873), the main object of the author was to prove that the annual work of secular contraction in our globe, when transformed into heat, was more than adequate for the supply of volcanic activity existing upon our planet. While indicating generally the circumstances which must attend as results of the descent of the exterior shell

* From the Philosophical Magazine for July, 1875.

upon the more rapidly contracting nucleus, it was not necessary to his argument to follow into detail the mechanism of local dislocation and crushing due to such descent. Nor would the limits of his paper admit of his entering into much detail as to the circumstances attending subterranean dislocation and crushing of rocky matter, or pointing out how some of these must greatly tend to exalt the temperatures due to the transformation of the mechanical work locally done. It was necessary to a truthful examination of the question whether or not the annual supply of heat transformed from the work of secular contraction were sufficient to meet the demands of existing volcanic action, that he should not overrate the work so transformed; and accordingly, in determining by experiment a measure for the amount of that work, the author viewed the work of crushing of unconfined or unsupported masses alone as the source of heat, this method being that only which could afford perfectly trustworthy experimental results. He paid no regard to the additional work that must attend the collision and friction of already crushed masses in the further progress of their deformation and forced transport to points more or less distant from those at which the crushing had taken place. The work of crushing in free air was capable of rigid determination; the work of subsequent deformation and transportation can only admit of estimation upon assumed data, and these necessarily of a somewhat arbitrary character, seeing how little we know accurately of the nature and disposition of the rocky materials of our earth's crust, except at the most inconsiderable depths from its surface. Nor were any of the circumstances pointed out by which high temperatures are capable of being attained locally in rocky masses crushed beneath our surface and which we must assume as those actually occurring in nature. The writer's object here is to point out, 1st, that, taking the annual supply of heat from transformed work of contraction, by experiment in the way he has done, the result, though more than sufficient to sustain his theory, affords alone no complete measure of the highest temperature that may through its means be locally developed; 2ndly, to answer some doubts which have been raised as to whether the temperature to which subterranean rocky masses can become raised by the heat evolved in their crushing and transportation of particles can be sufficient to bring more or less of these at such foci of crushing and dislocation to the fusing-point of such materials, which the author in his original paper assumes to be 2000° Fahr.

Professor Hilgard, occupying the chair of geology in the University of Michigan, U. S., in an able paper published in

the American Journal of Science, vol. vii, June, 1874,* has, in terms as clear as they are courteous, pointed out the *lacunæ* in the author's original paper in the following passage:—

"One point, however, must strike every reader of the original memoir, viz: the preëminence given by Mallet to the *crushing of solid* rock as the means of producing heat and fusion. One would naturally look to the results of his experiments on this subject for the proof of the efficiency of this agency. But we find that the maximum *temperature* resulting from the crushing to powder of the hardest rock is something over 217° Fahr. This, then, represents the maximum increment of temperature that can be rendered efficient toward the fusing of rocks by the crushing process under the most favorable circumstances, viz: upon the supposition that it takes place instantaneously, or under such circumstances that the heat cannot be conducted away, and, further, that the resistance of the rock has not been materially diminished by the downward increase of hypogeal temperature. At the most moderate depths at which volcanic phenomena can be supposed to originate the last-mentioned factor must exert a very considerable influence, reducing materially the available heat-increment. Hence the numerical results of Mallet's laborious experiments on rock-crushing, however interesting and useful as affording a definite measure of the thermal effects producible by this means, yet fail to carry conviction as to the efficacy of this particular *modus operandi* in reducing large masses of solid rock to fusion, unless essentially supplemented by friction, not so much of rock walls against each other, but more probably by the heat produced within more or less comminuted *detrital* or *igneoplastic* masses by violent pressure and deformation.

"It may be doubtful what would be the physical and thermal effect of enormously great pressure upon rock powder such as was produced in Mallet's experiments; but it would seem that if *made* to yield, the frictional effect must produce very high temperatures. *A fortiori*, solid detrital masses of variously sized fragments intermingled (such as, rather than powder, would be likely to result from steady pressure), yielding rapidly under great pressures, might, under the *combined influence of friction and rock-crushing*, well be supposed to reach the temperature of fusion, which a simple crushing of a solid mass by pressure would have failed to produce. Mallet mentions the probable influence of friction and of the squeezing of igneoplastic masses, but does not attach to these agencies such importance as they seem to me to deserve.

"Of the complex thermal effects of the movements of

* Phil. Mag., July, 1874, p. 41.

detrital masses under great pressure Mallet's figures of course offer no measure whatsoever; nor is this, or even the thermal coefficients resulting from his rock-crushing experiments, at all necessary to the establishment of the postulates of his theory."

Subsequently the Rev. O. Fisher, in a paper read before the Geological Society of London, May 12, 1875, entitled "Remarks upon Mr. Mallet's Theory of Volcanic Energy," has repeated the observations of Professor Hilgard, and extended his objections to the author's theory in general in a way which appears not warranted. It will be sufficient here to quote the following from Mr. Fisher's paper:—"Indeed the form in which the objection to Mr. Mallet's reasoning suggested itself to my mind on first reading his paper was simply this. If crushing the rocks can induce fusion, then the cubes experimented upon ought to have been fused in the crushing; and I still adhere to this simple mode of expressing my objection." Again:—"He considers that the heat so developed may be localized, and that the heat so developed by crushing, say 10 cubic miles of rock, may fuse 1 cubic mile. But I ask why so? The work is equally distributed throughout; why should not the least be so also? Or if not, what determines the localization? For example, suppose a horizontal column 10 miles in length and 1 in sectional area to be crushed by pressure applied at its ends, which of the 10 cubic miles is the one to be fused? But if no cause can assign one more than another, it is clear that they will all be heated by 170° and none of them fused."

If a cube of rock, which in free air is found to crush under a certain pressure, be imagined situated deep within a mass of similar rocks and there crushed, it does not admit of dispute that the work necessary to effect crushing must be largely increased; the particles of the cube and the entire mass of surrounding rocks are under the insistent pressure of the superincumbent rock in a state of elastic equilibrium. It follows, therefore, that the pressures of the surrounding rock produce the same effect upon the cube as regards resistance to crushing as if they were cohesive forces acting within the cube; and the work necessary to crush the cube by its finally giving way, in whatever direction this *encastrement* by pressure may be least, will be increased over that which would crush it in free air nearly in the ratio in which the imaginary cube is exposed to external pressure greater than that in air. Thus, if the cube of Guernsey granite (No. 12, Table I, Phil. Trans., part 1, 1873, p. 186) which required 4,336,712 lbs. per square foot to crush it in air, equivalent to a superincumbent column of the same rock of the mean specific gravity 2·858, or weighing 178·3392 lbs. per cubic foot, be supposed situated at a depth of ten to

twenty statute miles, it will require rather more than 2·14, or, at twenty miles, 4·28 times as much pressure upon two opposite faces to crush it that it did when in air; and if we assume the displacement of the crushed particles after crushing to be the same as in the case of the cube crushed in air, then the work and the heat due to its transformation will be also 2·14, or 4·28 times as great. And, as in the case of the cube crushed in air the heat developed was sufficient to fuse at (at 2000° Fahr.) 0·108 of its own volume, or, in other words, the crushing of 10 cubic feet of the rock would be required to raise to that point one cubic foot, then in the case of the imaginary cube situated at the depth of ten miles enough heat would be evolved by the work of crushing each cubic foot to fuse 0·231 cubic foot, or, at twenty miles, to fuse 0·462 cubic foot of the same rock, or nearly half the volume crushed,—and this assuming that the initial temperature of the rock at 10 or 20 miles depth was only 57° Fahr. as in the author's experiments, instead of from 500° to 1000° Fahr. or more as it may be at 10 to 20 miles depth. Therefore, under the pressure due to a depth of 20 miles and an initial temperature of 1000° Fahr., the heat developed by the work of crushing each cubic foot of rock will be sufficient to fuse its own volume. Thus also if we assume the fusing-point of the rocks not to be 2000° Fahr., as indicated by the author's experiments on the cooling of slags, but considerably higher, say 2500° or more, we have still a sufficient supply of heat due to crushing alone to bring 0·8 of the entire volume to the fusing-point.

These considerations, apart from all others yet to be adverted to, appear fully sufficient to refute the Rev. O. Fisher's first objection above quoted; indeed the statement that if under any circumstances and in the rock-masses of nature "crushing can induce fusion, then the cubes experimented upon ought to have been fused in the crushing," seems as unsupportable as it would be to affirm that no heat is developed by the slow oxidation (*eremacausis*) into water and carbonic acid of a pound of wood, which when burned develops a well-known amount of heat.

The depths above assumed do not widely differ from those at which the foci of earthquakes have been found by the author (Report on Neapolitan Earthquake) in 1857, and by others since that time, and which may be presumed to indicate in some degree the possible depth of volcanic activity.

The writer now proceeds to reply to the second objection of the Rev. O. Fisher as above quoted, which appears to him based entirely on a misconception of the physical conditions involved. Let us consider what will happen in the case of a prism or column of rock crushed against the face of an unyield-

ing mass. If the prismatic mass be not homogeneous throughout, crushing will commence at the weakest place; if it be perfectly homogeneous, crushing will commence and continue where the prism is in contact with a fixed mass, and that whether the prism be crushed at one or both ends—because it is at such surface of contact that the compression of the particles of the prism is greatest, and where therefore the elastic limit of their cohesion is first and successively overpassed. This may be seen illustrated in the stonework of buildings the material of which is overloaded, where crushing or spalling off of the ashlar stones only occurs at and near the joint.* In either case, whether the prism be homogeneous or not, the crushing must be localized either to the end or ends of the prism, or to the plane of weakness where it first yields, and which then becomes the crushing surfaces of two opposed prisms. It is these physical conditions which "determine the localization" of crushing in the prism, and which conditions have been disregarded in the Rev. O. Fisher's objection. Let us now consider the subsequent effects of the successive crushing of a column of prismatic mass of rock, one extremity of which is continually urged against the face of a fixed mass of rock which does not yield, a case which approximates to that which most frequently occurs in nature, and which, to fix our ideas, we may suppose presents a face for crushing one square foot; and being continually urged forward, and the pressure being greatest where the pressing column comes into contact with the fixed mass of rock, the extremity of the column supposed homogeneous, or the parts adjacent thereto, are continually crushed by a succession of *per saltum* movements. The first cubic foot of the column that is crushed has its temperature raised, let us suppose, by the minimum of 217°. The crushed fragments at this temperature are pushed aside by the advancing column, whose extremity is thus surrounded by crushed materials at a temperature of 217°, and the second foot in length of the column becomes crushed. But the material of this second cubic foot is at a higher temperature before it is crushed than was the first cubic foot; so that the heat due to the transformed work of crushing of each successive cubic foot of rock raises its temperature to a higher point than that of the preceding one, because each successive cubic foot at the instant before crushing is at a temperature already higher than the preceding ones, resulting from the heat taken up by the uncrushed column from the hotter portions of material surrounding it that have already been heated by

* See also E. Hodgkinson's experiments on the directions of fracture of crushed material, Brit. Assoc. Report, vol. vi; and Tredgold on Cast Iron, by Hodgkinson, part 2, p. 319, and plate 1.

crushing; so that, if T be the temperature produced in the first cubic foot crushed, and t be the temperature of the crushed material which communicates a portion of its heat to the next cubic foot crushed, the temperatures of successive cubic feet crushed may be illustrated by some such series as the following:—

Cubic feet crushed.

No. I.	No. II.	No. III.
T	$T+\frac{t}{n}$	$T+\frac{t}{n}+\frac{t}{m}\ldots$ &c.

We have here supposed the column crushed at atmospheric pressure; but if crushed under an insistent column of 20 miles, then the temperature T would be 4·28 times 2·17°=928°, and the subsequent temperatures correspondingly increased.

No limit arises to this continual augmentation of temperature while the rock retains its rigidity: after that has been seriously impaired or lost, any further exaltation of temperature apart from the detrusion or transport of fragmentary matter, as hereafter referred to, becomes dependent upon the deformation and detrusion of a more or less plastic mass. It is well ascertained, however, by observation on a great scale, that granite remains rigid at a temperature nearly approaching the softening point of cast iron;* so that a large range of rigidity must exist for the exaltation of its temperature in the way above suggested; and in the state of aggregation which we are warranted in supposing rocky masses to exist in at considerable depths, it is probable that this range of rigidity would be even further extended than in the case of granites found at or near the present surface of our globe.

There is a close analogy between the conditions of gradual exaltation of temperature above sketched, and those by which aërolites, flying at an immense velocity through our atmosphere, are heated from the temperature of the stellar spaces to that of incandesence or even fusion of those bodies. The aërolite, which, according to Schiaparelli, may in some instances be forced through our atmosphere with a relative velocity exceeding 3500 feet per second (one enormously exceeding that at which air can rush into a vacuum), compresses the stratum of air immediately in advance in almost the same manner as if at the first instant of contact the air were a rigid body. The temperature developed is greater as the velocity of compression is so, and as the volume

* The observations upon which these statements are founded have been made after various great conflagrations of stores or warehouses at London, Liverpool, and Dublin, into the construction of which granite blocks and cast iron in columns, girders, &c., largely entered. The cast iron was either melted or softened to the consistence of soap; the granite heated to like temperature, except being split in various directions, was found unaltered, except more or less in color, after having been again cooled.

compressed is less; the most highly heated air is therefore the stratum directly in contact at any instant with the stone; and the latter licks up more or less of the heat as it passes through a succession of such compressed strata, and so receives continual accessions of heat until the temperature of the meteoric stone itself reaches the limit given by that of the stratum of compressed air in immediate contact with it at any instant. If a body as mobile and compressible as air can thus by sufficiently rapid compression be raised above the temperature of incandescence, we may readily conceive how great an exaltation of temperature may be produced in the rigid materials of our earth's crust when exposed to a pressure which may be viewed as limitless in reference to the resistance opposed to it, and which, in consequence of the conditions of elastic resilience hereafter referred to, may give rise to motion and crushing with velocities even exceeding those with which aërolites traverse our atmosphere.

The well-known experiment of cutting a hard steel file in two by the rapid rotation of a thin disk of soft sheet iron pressed against it is another example. The heat developed at the working-point, so far as it is communicated to the disk, is rapidly carried off and dissipated by its rotation, and it thus remains cool enough to be touched by the hand, although the heat developed by it and accumulated at and near the working-point in the file is sufficient to raise that to the temperature at which cast steel becomes softened and approaches fusion.

The cutting of steel railway bars across when at a very low red heat by a rapidly revolving circular saw, which revolves partially immersed in cold water, and from whose action a torrent of incandescent fragments of steel is discharged, is a like case.

Besides the heat transformed from the work of compression and crushing, a large amount of heat must also be generally produced by transformation of the work expended in friction and detrusion. No experiments have as yet, to the author's knowledge, been made upon the amount of heat developable in fragmentary pulverulent masses, such as sand, by the forcible transposition of more or less of the particles; nor do we know with certainty the conditions under which external mechanical pressure is transmitted through sand or like discontinuous matter. As in rigid solids exposed to unequal mechanical pressures there exist planes or surfaces within the mass such as have been denominated by Moseley "planes of easiest shearing," or sliding, so in masses of pulverulent matter, whatever be the shape or size of the particles, provided these be small in relation to the whole mass, and their mutual adhesion (if any) small also, such planes must by unequal mechanical pressure be brought into existence. Along any such plane we may imagine the

sand or other pulverulent matter forced to move over itself in opposite directions at opposite sides of the plane; that is to say, we may suppose the sand forced along such a plane much in the same way that a mass of sandstone or of granite would be forced along such a shearing plane as had been produced in it previously by mechanical pressure. If this reasoning be admitted, we must suppose that heat would be developed along such a plane and at short distances from it in a way more or less analogous to that produced by forcing one rough surface of stone over another. What the coefficient of friction in this case would be can only be determined by experiment; but we may justifiably conclude that the amount of friction per unit of surface would increase proportionately to the pressure applied externally to the entire mass—exposed to more or less of which, motion at any such surface of friction must take place. Coulomb, Morin, and others have found the friction of some sorts of rough stone upon other rough stone to reach as much as three-fourths the pressure; and should this coefficient increase proportionately under the enormous pressures to which a discontinuous mass at several miles depth may be subjected, we can readily see that the transformed heat of friction produced by internal movements taking place in such materials after crushing has occurred, must be the source of a large amount of heat over and above that originally due to the crushing itself. Thus, for example, if we assume a surface of such disintegrated material sliding over a similar surface, or over a rough surface of coherent rock, and under the pressure of ten miles of rock of the specific gravity of granite, at the rate of one foot per second, and if we take the coefficient of friction as low as 0·5, we have 4,326,600 foot-pounds of frictional work per second, which, divided by J (=772), gives 5604 units of heat evolved per second from each square foot of surface; and to this development there is no limit while the circumstances continue the same, except that of the distance that one surface is forced over the other. And great as is this evolution of heat under such enormous pressures, it would be further increased in the event of the fragmentary particles being heated so as to present incipient viscosity of surface and more or less of mutual agglutination.

Temperature with respect to any given solid material is dependent upon the units of heat present in a unit of mass or of volume of the substance. If for the same total heat we diminish the mass or volume, the temperature is proportionately increased. When the material is surrounded by matter capable of carrying off heat by conduction, or evection, or radiation, and the heat is evolved within the mass by work done upon it, then another condition, that of time, has to be taken into account: for the shorter the time within which a given amount of

heat due to transformation of work is evolved within the unit of mass, the less of that total is dissipated by conduction, &c.

A familiar example of this is every day seen in the light and heat elicited by abrasive friction, or collision between hard bodies. When two lumps of granite or other hard stone are struck together, heat and light are instantly evolved at the small surface of contact, where the material of one or both masses is crushed. The work done may be but that of the crushing of a fraction of a grain of the stone; but it is great in reference to the extremely brief instant of time during which the work is performed: the crushed particles are raised to a temperature of luminosity for a brief moment because there is not time for the surrounding surfaces of the cold stone to carry off the heat evolved by conduction, though the dissipation of heat thus produced is such that the luminosity again instantly disappears. The temperature at the crushing-point is greater as the work done in a unit of time and upon a given weight of the material is greater. That the temperature capable of being thus produced approaches that of the fusing-point of steel, is of a gun-lock by the flint. In the case of small masses of rock, evident from the phenomenon of a spark struck from the steel such as the $1\frac{1}{2}$-inch cubes of the author's experiments, crushed between two opposite surfaces of steel, the actual temperature of the crushed particles can never be found to reach that due to the work of crushing; for the heat of relatively small mass of the crushed cube in close contact with far larger masses of cold steel of high conductivity is carried off almost as fast as it is evolved; and as the total amount of heat evolved from the crushing of such a cube of the hardest rock experimented upon by the author (namely, number 12, Table I, Phil. Trans., part 1, 1873, p. 186) could raise its own mass only through 217°, if the temperature of fusion of the rock may be taken at 2000°, it is obvious that such a cube could not be fused by the work of crushing alone, even though all the heat due to the crushing work remained in the cube, none being dissipated to surrounding objects.

In the case of a cube such as this losing heat by dissipation, the temperature of the crushed mass depends upon the time in which the work of crushing is done. In the author's experiments the crushing of each cube in column 12 occupied a mean time somewhat greater than that in which a heavy body could fall freely through a space of 0·09 foot (No. 12, Table I. *l. c.*, col. 19)—that is, 0·075 of a second: for more rapidly than that the crushing surfaces could not approach each other. If, however, the conditions had been such that but $\frac{1}{n}$-th the above time were expended in the crushing, then a proportionately less quantity

of the heat evolved would have been dissipated; and this, we shall see further on, must be the case in nature. When two rock-surfaces are urged against each other in the shell of our globe by the gradual withdrawal of support by contraction of the nucleus, the rocky masses for great distances from the opposed surfaces are brought into a state of elastic compression, gradually increasing up to the crushing-point somewhere, when a greater or less portion of rock suddenly gives way by crushing and is more or less removed by detrusion in some lateral direction. The material of the rock for a greater or less distance from the crushing-point is therefore in the condition of a compressed spring which is suddenly released. When so released, the velocity of resilience depends principally upon the modulus of elasticity of the rock; and the velocity of release of the spring, which is that with which the crushing is performed, is extremely great in the case of hard granite or generally similar homogeneous rock, in which it probably exceeds 10,000 feet per second, though in very much less-elastic rocks falling considerably short of this.* But had the rock-specimen crushed in the author's experiments been a cubic foot in place of an inch and a half cube, the time of crushing must have been *greater* than ·249 second (or nearly a quarter of a second), or the crushing would have been performed with nearly a velocity of 2 feet per second, and that would be less than $\frac{1}{5\,0\,6\,5}$ of the velocity with which the same would have been crushed if circumstanced as in the shell of our globe. And if we extend our view from the crushing of a cubic foot or two to that of a cubic mile or more, we see that there would be very little of the total heat evolved lost by dissipation, there being scarcely any time in which that could occur, the possible rate of crushing of a cubic mile being less than half a second.

In the case of a very large mass of rock crushed simultaneously, or nearly so, as every portion of the rock evolves heat proportionate to the crushing-work done upon it, so the heated portions of crushed material situated near the exterior of the entire mass act as "*jacketing*," so as to preserve the deeper-seated portions of the heated mass almost absolutely from any dissipation of heat for a considerable length of time, that time depending, *cæteris paribus*, on the conductivity of the crushed material and the difference in temperature between the crushed material and the uncrushed rock adjacent to it. There are some experimental grounds for concluding that the absolute crushing-force per unit of volume in any given rock increases as the absolute volume of the mass simultaneously crushed becomes greater; thus it has been found by Rondelet that large cubes of stone required a proportionately larger crushing-force than

* See the author's "Earthquake-wave Experiments," Phil. Trans., 1862, vol. clii.

smaller ones; much stress, however, cannot be laid upon this, as we cannot assume with any certainty what are the precise conditions under which rock-surfaces in our earth's shell are forced together, and the distribution of the crushing-pressure may be indefinitely varied.

In the author's experiments the cubes crushed by pressure on two opposite faces were free upon the other four; it cannot be doubted that, had only two opposite faces been free and the pressure applied simultaneously upon the four other faces, two and two respectively, the pressure necessary for crushing or the work thereof would have been considerably increased. Further, if none of the faces were free, and all those except the two opposite faces to which the crushing pressure is supposed applied, had the motion outwards of any of their particles opposed by an initial pressure, such as that of an insistent mass of rock, the work to be done to produce crushing would be necessarily increased by the amount required to overcome such initial pressure, as has been already pointed out.

Another and further source of heat arises after crushing and detrusion of the fragmentary matter, and after the latter has arrived at a temperature at which the fragments have become more or less viscous and adherent by reason of the further work expended in the deformation and detrusion by forcing forward through highly irregular or constricted rock-channels of the heated and now viscous mass. There do not exist at present sufficient data by which to calculate the amount of work necessary to a given amount of deformation in viscous masses; and hence we cannot calculate the amount of heat that in nature might arise from it. Hirn, however, has shown that in the case of plastic bodies, such as lead, the heat developed is proportionate to the work done in deformation; so that, if we knew the pressure per unit of surface necessary to produce a certain deformation in an already heated mass of given viscosity, we could calculate how much its temperature would be exalted by the work of the assigned deformation.

Examples, however, are not wanting which prove that a very large exaltation of temperature can thus be produced—as, for example, in the old-fashioned method by which blacksmiths were accustomed to light their fires. A thin square rod of very good tough iron was hammered at its extremity by a succession of rapidly given blows from a light hand-hammer. After a minute or so the rod, for the portion of its length hammered at the extremity, became red, and in a second or two more of distortion of its form by continuance of the hammering, the iron could be made nearly white hot.

A similar example, on a much larger scale, occurs in the process of rolling iron or steel. When a heavy billet of iron or

steel, heated to a brilliant yellow heat, is passed between the rolls of the iron mill, and the massive lump is rapidly elongated into a bar, its temperature, notwithstanding that it is rapidly and constantly losing heat by radiation and evection, is observed visibly to increase, so that the mass becomes at a certain stage white or welding hot by the transformation into heat of the work of deformation so rapidly and powerfully applied to it.

The action of the machine employed in the arsenal at Woolwich for making lead rods to be afterward pressed into bullets affords another striking example. In this machine a cylindric block of lead, maintained at a temperature of 400° Fahr., is by a steady pressure upon the end, which is 8″·5 in diameter, of 16,700 lbs. per square inch of its surface, forced through an aperture at the other extremity into a rod of 0″·525 diameter, at such a rate that five inches in length of the cylindric block becomes a rod of about 100 feet in length of the above diameter per minute. We have thus 393,906 foot-pounds of work done upon the lead per minute, dividing which by J we have 510·2 British units of heat developed per minute from the transformed work. In the actual machine the whole of this is ultimately dissipated and lost; but if none of it were dissipated, as the cylindric block of lead of 8·5 in. diameter by 0·416 ft. (5 inches) weighs 116·3 lbs., and the specific heat of lead is =0·029 (or perhaps a little more at 400° Fahr.), it follows that the heat developed by its deformation from the short cylindric block of 5 inches length to a rod of about 100 feet length, is enough to raise the temperature of the lead through 151° Fahr., or, were no heat lost, to raise its temperature from 400° to 551° or thereabouts—that is, within about 50° of its melting point. If, therefore, we could by a reverse process squeeze the 100-foot rod back into the original block of 8″·5×5″, we should find the lead in the latter not only liquid, but considerably above its temperatue of fusion, or at nearly 700° Fahr. It is obvious, therefore, that any viscous or plastic body such as lava, continually forced through apertures varying in area and form and suffering continual deformation, as when forced through a volcanic tube or vent, must have its temperature continually exalted so long as it continues thus to be urged forcibly forward, assuming, as is very nearly the truth in nature, that an extremely small proportion of the heat developed in the process can be dissipated by conduction to the walls of the tube.

The preceding remarks appear to the writer sufficient to show that there is no physical difficulty in the conception involved in his original memoir (Phil. Trans. 1873), but not there enlarged upon in detail, that the temperatures consequent upon crushing the materials of our earth's crust are sufficient locally to bring these into fusion.

Art. XXXV.—Sir Charles Lyell.

No European geologist was so well known, personally, in the United States as Lyell. His two visits to this country, in 1841 and 1845, recorded in four volumes of travel characterized by great good judgment, large mindedness and catholicity, made his name familiar throughout the land, and gave a degree of popularity here to his philosophical and technical writings which they would otherwise have hardly obtained.

Called by Mr. Lowell to Boston in 1841 to deliver a course of twelve lectures on Geology before the "Lowell Institute," Lyell was the first European, of eminence in science, who appeared upon the platform as a lecturer before an American audience. That his lectures were highly esteemed is well known, and it was a sufficient evidence of this that he was again invited to Boston on a like commission in 1845–6, and before the same institution. The personal relations and friendships commenced on these occasions endured to the end, and were rendered doubly interesting by the charm shed over every social relation by Lady Lyell, who won universal esteem by those qualities of manner which were less prominent in her often abstracted husband. The following familiar private letter from the late Dr. Mantell, written in 1841 to Prof. Silliman (the elder), gives a vivid sketch of Lyell as he appeared to his scientific associates at the time of his first visit to the United States. As all the parties named in this letter are now passed away there can be no objection to its reproduction in this connection.

"London, *June* 14th, 1841.

"My very dear friend:

"I was about to write you to inform you of Mr. Lyell's intentions which he communicated to me but a short time since. I dined with him last week—a farewell party. His charming little wife, a daughter of Mr. Leonard Horner, accompanies him. I have said so much of you and yours to her that she is quite anxious to visit New Haven; if she does I am sure you will be delighted with her. And now for a strictly private sketch of my old friend. About twenty years or more ago, one beautiful summer evening, a young Scotchman called at Castle Place (Lewes*) and announced himself as Mr. Lyell, stating that he was fond of geology, had been attending Jameson's lectures at Edinburgh, and had visited his former Alma Mater, Midhurst Grammar School, in the west of Sussex; and that, while rambling about the

* Mantell's place of residence at that time.

neighborhood, he found some laborers quarrying in stone which they called whin. As this term is *Scoticé* trap, the young traveler was much puzzled to know how such a rock appeared in the south of England, and upon inquiry of one of the laborers why the stone was so called, the man referred him to 'a monstrous clever mon as lived at Lewes, a doctor who knowed all about them things and got curiosities out of the chalk pits to make physic with.' The man, in short, had been formerly a Lewes quarryman, and one of my collectors. Mr. Lyell being alone and on horseback and having nothing better to do, rode gently over the South Down, some twenty-five miles, and at the close of the day found himself at my residence. We were mutually pleased with each other; my few drawers of fossils were soon looked over, but we were in gossip until morning, and then commenced a friendship which has continued till now.

Mr. Lyell was educated for the bar. He practiced on the western circuit seven or eight years, and he allowed me to correspond with him only during the vacations. His father, who is a Scotch Laird, is still living, and there are several sons and daughters. Mr. Lyell is the eldest, and at the death of the father inherits the family estate, which, I believe, is moderate. However, about seven or eight years after our acquaintance, Mr. Lyell with great good sense, abandoned his profession, with his father's consent, and devoted himself wholly to geology, content with a moderate income, and living in a very unostentatious manner in an unfashionable part of the city. A few years ago he married Miss Horner, who is much younger than himself (Lyell is 45 or 46), and a more suitable companion he could not have found. He has no children. In person, Lyell presents nothing remarkable except a broad expanse of forehead. He is of the middle size, a decided Scottish physiognomy, small eyes, fine chin and a rather proud or reserved expression of countenance. He is very absent, and a slow but profound thinker. He was Professor in King's College, London, and gave lectures there and at the Royal Institution, but it so happened that I never heard him lecture. He always takes part in the discussions at the meetings of the Geological Society, but he has not facility in speaking; there is hesitation in his manner, and his voice is neither powerful nor melodious, nor is his action at all imposing. As a popular lecturer he would stand no chance with Buckland or Sedgwick. He is providing himself with very beautiful illustrations for his lectures at Boston; and I should suppose the prestige of his name and his European reputation will insure him a flattering reception. * * * * There is a hauteur or reserve about Mr. Lyell to strangers that prevents his being

so popular among our society as he deserves to be. I believe him to have an excellent heart, and he is very kind and affectionate when his better feelings are called upon." * * * *

This criticism of Lyell's style and manner as a public speaker was certainly well founded, as all will agree who ever heard him lecture. But despite all infelicities, so great was the value and richness of his matter, that he commanded the most respectful and interested attention from his auditors. The reader of his "Principles" could not fail, however, to be struck with the fact that the classic elegance of Lyell's style, for which his more important productions are so justly celebrated, must have been the result of much labor.

We cite from the Geological Magazine edited by Henry Woodward, F. R. S., the following notice of his life and labors. A more elaborate memoir may be expected in the next annual address of the President of the Geological Society of London.

"On Monday, the 22d of February, at his residence in Harley Street, and in his seventy-eighth year, Sir Charles Lyell, after a long life of scientific labor, passed peacefully from amongst us, to his honored rest.

"To the outside world it may seem strange that the death of a man who was neither statesman, soldier, nor public orator, should arouse our sympathies so strongly, or that he should be so highly esteemed all over the world; but geologists know well what Lyell has done for them since he published the first volume of 'The Principles of Geology' in 1830.

"It is in the character of historian and philosophical exponent of geological thought that Lyell has achieved so much for our science; nor can we fail to remember that those clear and advanced views, for which he became so justly celebrated, were advocated by him forty-five years ago, at a time when scientific thought was still greatly trammelled by a strong religious bias, and men did not dare to openly avow their belief in geological discoveries nor accept the only deduction which could be drawn from them.

"Born at Kinnordy, his father's seat near Kerriemuir, in Forfarshire, on the 14th of November, 1797, Lyell received his early education at a private school at Midhurst, and completed it at Exeter College, Oxford, where he took his Bachelor's degree in 1819, obtaining a second-class in Classical honors in Easter Term. On leaving the University, he studied for the Bar, but never practised that profession, his tastes having been led by Dr. Buckland's lectures to the study of geology as a science. In 1824 he was elected an Honorary Secretary of the Geological Society of London, of which he was one of the ear-

liest Fellows. On the opening of King's College, London, a few years later, he was appointed its first Professor of Geology. He had already contributed some important papers to the 'Transactions' of the Geological Society, including one 'On a Recent Formation of Freshwater Limestone in Forfarshire, and on some Recent Deposits of Freshwater Marl, with a comparison of recent with ancient Freshwater Formations, and an Appendix on *Gyrogonites*, or Seed-Vessels of Chara;' also one 'On the Strata of the Plastic Clay Formation exhibited in the Cliffs between Christchurch Head, Hampshire, and Studland Bay, Dorsetshire;' another 'On the Freshwater Strata of Hordwell Cliff, Beacon Cliff, and Barton Cliff, Hampshire;' and an elaborate paper on the 'Belgian Tertiaries.' In 1827 he contributed to the *Quarterly* a review of Mr. Poulett Scrope's 'Geology of Central France' (the perusal of which is said first to have stimulated him to prepare and publish 'The Principles of Geology' on which his reputation as a philosophical writer mainly rests). These lesser works all showed a power of observation and of generalization which prepared the learned world for some greater and more important treatise from his pen, which should deal, not with local details, but with the general principles of the science. Nor were they disappointed when his *magnum opus*, 'The Principles of Geology,' appeared in three successive instalments, published respectively in 1830, 1832, and 1833. The work, subsequently enlarged into two volumes, has passed through numerous editions, and is still in as much demand as ever among students of the science. The work was subsequently divided into two parts, which have been published as distinct books, viz. 'The Principles of Geology, or the Modern Changes of the Earth and its Inhabitants, as illustrative of Geology,' and secondly, 'The Elements of Geology, or the Ancient Changes of the Earth and its Inhabitants, as illustrated by its Geological Monuments.' The substance of the last-named work has also been published under the title of 'The Manual of Elementary Geology,' a French translation of which was issued under the auspices of the famous Arago.

"Already, some time previous to the publication of this work, Mr. Lyell had been chosen a Vice-President of the Geological Society; and in 1828 he had undertaken a journey into the volcanic regions of Central France, visiting Auvergne, Cantal, and Velay, and continuing his journey to Italy and Sicily. He published the results of this expedition in the 'Edinburgh Philosophical Transactions,' and also in the 'Annales des Sciences Naturelles.'

"It was, however, the publication of his 'Principles of Geology' that gave him that established reputation which he

ever since continued to enjoy. 'Which of us,' asked Prof. Huxley, in his Anniversary Address to the Geological Society in 1869, 'has not thumbed every page of the "Principles of Geology?"' And he adds, 'I think that he who writes fairly the history of his own progress in geological thought will not easily be able to separate his debt to Hutton from his obligations to Lyell.' This cordial testimony of a fellow-laborer in the cause of scientific enlightenment exactly indicates Sir Charles Lyell's place in the history of that task. He was a man of singularly open mind, one of those who stand above their contemporaries and hail the dawn of new truths upon the world. His own works mark the progress of his own as well as of the public opinion on the great problems raised by scientific discovery, and he remained to the end of his life always ready for the reception of new facts, and for the corresponding modifications of opinion.

"Sir Charles Lyell had traveled and seen much. Thus in early manhood he explored many parts of Norway, Sweden, Belgium, Switzerland, Germany, and Spain, including the volcanic regions of Catalonia. In 1836 he visited the Danish Islands of Seeland and Monen, to examine their Cretaceous and Tertiary strata. In 1841 he was induced to cross the Atlantic, partly in order to deliver a course of lectures on his favorite science at Boston, and partly in order to make observations on the structure and formation of the Transatlantic Continent. He remained in the United States for a year, traveling over the Northern and Central States, and extending his journey as far southward as Carolina, and northward to Canada and Nova Scotia, his exploration ranging from the basin of the St. Lawrence to the mouths of the Mississippi. On returning from this journey, he published his 'Travels in North America,' a work of considerable interest to other persons besides geologists, and showing that he could extend his observations to the stratification of society around him as well as that of the earth beneath his feet. He paid a second visit to America in 1845, when he closely examined the geological formation of the Southern States and the coasts that border on the Atlantic and the Gulf of Mexico, and more especially the great sunken area of New Madrid, which had been devastated by an earthquake 30 or 40 years previously. Upon reaching England, he published his 'Second Visit to the United States,' a companion to his former work. For his other scientific papers we must refer our readers to the 'Proceedings' of the Geological Society, 1846–49, and its 'Transactions.'

"Late in life, about ten or twelve years ago, Sir Charles Lyell published another very important work, on 'The Antiquity of

Man,' summarizing and discussing all the important facts accumulated up to that time in favor of the high antiquity of the human race, viewed from the standpoints of the archæologist, the geologist, and the philologist.

"Numerous honors were conferred on Lyell in recognition of his services to science. As far back as 1836 he was elected to the Presidential Chair of the Geological Society, to which he was re-elected in 1850. He received from Her Majesty the honor of knighthood in 1848, and in 1855 the honorary degree of D.C.L. of the University of Oxford was conferred upon him. He had been for many years a Fellow of the Royal Society, and in 1833 received one of the Royal Society's Gold Medals for his 'Principles of Geology.' In 1858 the Royal Society conferred upon him the highest honor at their disposal—the Copley Medal; and in 1864–5 he filled the Presidential Chair of the British Association for the Advancement of Science. He received the Wollaston Gold Medal from the Geological Society of London in 1865 (his continued official connection with which had precluded his receiving it earlier). He was raised in 1864, on the recommendation of the then Prime Minister, Lord Palmerston, to a Baronetcy, which now becomes extinct by his decease. He was a Deputy-Lieutenant for his native county of Forfarshire.

"Sir Charles Lyell has been so long and so honorably known among the scientific teachers of the time, that though he had arrived at his seventy-eighth year, and the period of his chief intellectual and physical activity had long passed away, probably even the younger men of the present generation will feel that science is poorer by his loss.

"At the meeting of the Geological Society of London, held in the Society's room, Burlington House, Picadilly, on Wednesday last (February 24th), the President, John Evans, Esq., F.R.S., before commencing the business of the meeting, alluded to the great loss which all present had sustained. He little expected, when speaking on the last occasion, at the Anniversary Meeting, of the services which Sir Charles Lyell had rendered to science for the previous fifty years, that he should have on the present occasion to announce and lament his irreparable loss. Sir Charles Lyell had been a true philosopher and a sincere friend. He had lived to see the extension of science which he had so eagerly desired realized. In future times, wherever the name of Lyell shall be known, it will be as that of the greatest, the most philosophical, the most enlightened geologist of Great Britain or Europe.

"In accordance with the wish of the Council of the Royal Society, Sir Charles Lyell will rest beside his old friend and

fellow-laborer in science, Sir John Herschel, in Westminster Abbey."

We add the following appreciative remarks from Nature of March 4th.

"Lyell's claim to fame lies in this, that he organized the whole method of inquiry into the history of the formation of the crust of the earth, and established on a sound footing the true principles of geological science; his theory being that, by the uniform action of forces such as are now in operation, the visible crust of the earth has been evolved from previous states.

"Lyell was not only a keen investigator of natural phenomena; he was also a shrewd observer of human nature, and his four interesting volumes of travel in America are full of clever criticism and sagacious forecasts. His mind, always fresh and open to new impressions, by sympathy drew towards it and quickened the enthusiasm of all who studied nature. Had he done nothing himself, he would have helped science on by the warmth with which he hailed each new discovery. How many a young geologist has been braced up for new efforts by the encouraging words he heard from Sir Charles, and how many a one has felt exaggeration checked and the faculty of seeing things as they are strengthened by a conversation with that keen sifter of the true from the false!

"Though by nature most sociable and genial, yet Sir Charles often withdrew from society where the object of his life, the pursuit of science, was not promoted; but when anything interesting turned up he always tried to share his pleasure with all around. Many of us will remember the cheerful and hearty 'Look here'—'Have you shown it to so and so?'—'Capital, capital'

"The little wayside flower, and, from early happy associations, still more, the passing butterfly, for the moment seemed to engross his every thought. But the grandeur of the sea impressed him most; he never tired of wandering along the shore, now speaking of the great problems of earth's history, now of the little weed the wave left at his feet. His mind was like the lens that gathers the great sun into a speck and also magnifies the little grain he could not see before. He loved all nature, great and small.

"Much we owe to Leonard Horner, himself a good geologist, for having inspired the young Charles Lyell. In after years, when already well known, Charles Lyell chose as his wife the eldest daughter of his teacher and friend. Many have felt the charm of her presence—many have felt the influence of the soul that shone out in her face; but few know how much science directly owes to her. As the companion of his life, sharing his

labor, thinking his success her own, Sir Charles had an accomplished linguist who braved with him the dangers and difficulties of travel, no matter how rough; the ever-ready prompter when memory failed, the constant adviser in all cases of difficulty. Had she not been part of him she would herself have been better known to fame. The word of encouragement that he wished to give lost none of its warmth when conveyed by her; the welcome to fellow-workers of foreign lands had a grace added when offered through her. She was taken from him when the long shadows began to cross his path; but it was not then he needed her most. When in the vigor of unimpaired strength he struggled amongst the foremost in the fight for truth, then she stood by and handed him his spear or threw forward his shield. He had not her hand to smooth his pillow at the last, but the loving wife was spared the pain of seeing him die.

"It doubtless occurred to many a one among the crowd who saw him laid to rest among the great in thought and action, that he might have been eminent in many a line besides that he chose.

"His was a well-balanced judicial mind, which weighed carefully all brought before it. A large type of intellect—too rare not to be missed. But it was well that circumstances did not combine to keep the young laird on his paternal lands among the hills of Forfarshire: it was well for science that he was induced to prefer the quieter study of nature to the subtle bandying of words or the excitement of forensic strife. Failing health had for some time removed him from debates. Still to the last his interest in all that was going on in this scientific world never failed, and nothing pleased him more than an account of the last discussion at the Geological Society, or of any new work done. As a man of science his place cannot be easily filled; while many have lost a kind, good friend."

The number of *Nature* for August 26, contains an excellent portrait of Sir Charles Lyell, accompanying a biographical notice by Prof. Giekie.*

A list of Lyell's memoirs to the close of 1863 will be found in the Royal Society Catalogue, numbering, with his elaborate works, no less than seventy-one separate communications in his own name, and five more in connection with others.

* Artist's proofs of this portrait (engraved on steel by C. H. Jeem) may be had at the office of *Nature*, 29 Bedford Street, Strand, London, W. C. Price 5*s.* each.

ART. XXXVI.—*On the Arithmetical Relations between the Atomic Weights;* by M. D. C. HODGES.

THE group of elements, fluorine, chlorine, bromine and iodine, have, as their atomic weights, 19, 35, 5, 80 and 127 respectively. These numbers form a series, in which the difference between the succeeding terms is at first 16·5, then 44·5, and finally 47. The following groups of elements give similar series.

			Li=7	
C=12	O=16	N=14	Na=23	Mg=24
Si=28	S=32	P=31	K=39	Ca=40
	Se=79·4	As=75	Ru=85·5	Sr=87·5
	Te=128	Sb=122	Cs=133	Ba=137

The grouping of some of the above elements may be objected to, but there is no great change from that generally accepted.

If a group of such elements as copper, silver, cadmium, mercury, lead and bismuth is taken, the series of atomic weights gives a similar, but somewhat different result; in this case each term of the series is composed of several members.

Cu=63·4	(Zn=65·2)	The new difference, 96+, for the
Ag=108	Cd=112	higher terms occurs.
Hg=200	Pb=207	Bi=210

Zinc may be added to the first term, and corresponds to the second member of the second term. The lower terms are here wanting. Similar to these are the following:

Ru=104·4	Rh=104·4	Pd=107	
Au=196·7	Pt=197·4	N=197·4	Os=199·4

Many of the remaining elements may be placed in the following series:

Be=9·4	Ti=50	In=75·8	Y=68	V=51·3
Al=27·4	Nb=94	U=120	E=112·6	Mo=96
Th=231	Ta=182	Tl=204		W=184

Sn(=118) may be placed as a second member of the fourth term of the series containing arsenic and antimony.

There remain chromium, manganese, iron, nickel, cobalt, zirconium, cerium, lanthanum, didymium, hydrogen and boron. The position of these may be better seen in the annexed table, in which all the elements, with the exception of hydrogen, are arranged on lines, the succeeding elements in the same line differing in their atomic weights by a few units or parts of units, and those in different lines from the corresponding elements in the preceding or following line by one of the differences mentioned.

		2·6				·00125	·00143		·972	1·743
(1.)	Br 9·4	Bo 11	C 12	----	----	N 14	O 16	F 19	Na 23	Mg 24
	2·56		2·49			1·826	2	·003	·865	1·577
(2.)	Al 27·4	----	Si 28	----	----	P 31	S 32	Cl 35·5	K 39	Ca 40
					7·42	5·63	4·24	3·1872	1·52	2·542
(3.)	----	----	----	----	In 75·8	As 75	Si 79·4	Br 80	Rb 85·5	Sr 87·5
				7·29	18·33	6·7	6·183	4·95		
(4.)	----	----	----	Sn 118	U 120	Sb 122	Te 128	I 127	Cs 133	Ba 137
(5.)	----------									
(6.)	M 231	----	----	----	----	----	----	----	----	----
(1.)	----------									
						5·5	6·8	7·2	7·8439	8·8
(2.)	----	----	----	----	Ti 50	V 51·3	Cr 52	Mn 55	Fe 56	Ni 58
						8·64				
(3.)	Zr 89	Ta 90	Ce 91·2	Di 94	Nb 94	Mo 96	----	----	----	----
(4.)	----------									
(5.)	----------					19·13				
					Ta 182	W 184	----	----	----	----
(6.)	----	----	----	----	----	----	----	----	----	----
(1.)	----------									
	8·5					8·9		6·8		
(2.)	Co 59	----	----	----	----	Cu 63·4	----	Zn 65·2	----	Y 68
		11·2	12·1	11·4		10·57		8·7		
(3.)	----	Ru 104·4	Rh 104·4	Pd 107	----	Ag 108	----	Cd 112	----	E 112·6
(4.)	----------									
		19·5	21·5	21·13	21·4	13·59	11·86	11·4	9·9	
(5.)	----	Cu 196·7	Pt 197·4	Ir 197·4	Os 199.4	Hg 200	Th 204	Pb 207	Bi 210	----
(6)	----	----	----	----	----	----	----	----	----	---

The double line marks the positions possibly not occupied by any elements.

In this table all of the above series are given, and the series, consisting of several members for each term, also, find their place. When we start from Be=9·4 and notice the following elements in the same line, we find that the atomic weights continually increase till Mg=24 is reached; there are no more elements in the same line, the next highest being Al(=27·4) in the second line; the case is similar for this line, the atomic weights increasing to Ca(=40), the next above being in the third line. The difference between the first and second lines is about sixteen units; it becomes forty-four plus for the second and third, and third and fourth; and is, finally, about ninety-three for the last term of the series, from Ti, Nb, Ta to Be, Al, Th. These three differences do not seem to be constant, but to increase with the values of the atomic weights; this is most apparent for the largest; the smallest appears constant.

It would seem probable that the lower terms of the series after magnesium, and as far as aluminum, should be wanting; as, otherwise, there would be two elements of equal atomic weights and differing largely in their chemical natures; which last would be shown by their having to be placed in different parts of the table, which does not occur. From Ba(=137) to the next highest, Ta(=182) is forty-five units, about the value of the middle difference. If there were other elements between these, there would be a similar anomaly to that just mentioned. The last terms of the series having the largest difference for their last terms, are Ta(=182) to Th(=231); the increase in these is forty-nine units. It may be that Th(=231) is the largest atomic weight.

One advantage of the table is its bringing elements of similar chemical and physical characters together. The metals and metalloids are separated almost completely.

There are certain objections to the classification of some of the elements in the series in which they have been placed. It appears that the chemical properties do not always exactly correspond in the members of the same series; this is best shown in the case of the soluble oxides and sulphides; others are those of thallium and lead, and perhaps silver. The boundaries in neither of these cases are to be considered sharp, and in both are oblique.

There is a singular fact in regard to the specific gravities of the elements, when thus arranged. The specific gravities of the elements in the same line vary regularly and have one maximum and minimum; the maximum is in each case in the neighborhood of the members of the gold group, and the minimum among the metalloids; the positions of these two points are farther to the right, the lower the line; this is best seen for the minimum. The numbers above the symbols give the specific weights. Uranium is an exception.

From what precedes it appears: 1, that the atomic weights of elements of similar properties form series, the difference between the lower terms of which series being sixteen, for the middle terms forty-four to forty-seven, and for the largest eighty-eight to ninety-six; 2, that certain terms of some of these series may be wanting.

This law, together with the law in regard to the variation of the specific gravities in the different lines of the given scheme of the elements, could be used in fixing the value of a new atomic weight after its determination by analysis. The specific gravity being known, there could be two points equally distant from the maximum or minimum in each line, in which, in general, the element could be placed; the value as found by analysis, or one of its multiples, would satisfy one of these positions.

It is somewhat strange that there is no position for hydrogen in the table.

Art. XXXVII.—*On Southern New England during the melting of the great Glacier;* by James D. Dana. No. II.*

II. Absence of marine life from Long Island Sound through the Glacial and part of the Champlain periods.

The fact that the stratified estuary and seashore deposits of the New Haven region, and of other parts of the Connecticut coast bordering on the Sound, afforded me no trace of marine life was a puzzle so long as I looked upon them as true beach-made accumulations. I have continued my search at various localities, up to the present time, and in no case have I met with shells or other sea-relics. The evidence proves that the deposits are not of beach origin, but drift deposits made by the waters and gravel or sand of the melting glacier as they together hurried onward in floods to the sea, conjoined there with the action of the tidal currents and waves; and it establishes also, as I believe, that true shell-bearing sea-beaches do not here exist.

The same absence of marine products appears to characterize the drift along the northern shore of Long Island, or southern side of the Sound. The extended investigations of Professor Wm. W. Mather, while geologist of the State of New York, brought nothing of the kind to light; and my own recent examinations of the drift on the Island have been as barren of discovery. Moreover, I could gather no evidence from persons living in

* For No. I. of this Memoir, see page 168.

the sea-border towns that any shells had been found in the drift deposits *

Long Island Sound—the strip of water, 100 miles long and 5 to 20 miles wide, separating Long Island from Connecticut—is a very shallow trough. Its depth from its western limit to the mouth of the Connecticut river nowhere exceeds (as the Coast Survey charts show) 170 feet, and in general is between 75 and 100 feet; and through the small eastern portion, east of the Connecticut, it is for the most part under 200 feet. The depth was probably less than this in the Glacial era, since there is strong evidence that the water line was then at least 100 feet below its present level. However this be, so shallow a trough should have been occupied throughout with ice, if the glacier over Southern Connecticut were 1 500 feet in thickness, or even 1,000 feet; moreover the trough would have been the course at bottom of one of the largest of New England subglacial streams, since all the subglacial rivers passing through the State of Connecticut, including the chief river of New England, the Connecticut, would have been its tributaries. As the Champlain period opened the land subsided, giving greater depth of water to the Sound than now exists. But, with the melting going forward, the great stream of the Sound would have been swollen immensely in volume, and finally have borne icebergs seaward.

Such being the glacial conditions, the continuance of molluscan, articulate and verbetrate marine life in the Sound, like that now existing there, would have been impossible, and an extermination nearly or quite complete of the old fauna must hence have taken place.

After the melting of the glacier had ended, the return of the old species to the Sound sooner or later began; and it was probably accomplished before the Champlain period—the era of low level and great rivers—ended; but, with no elevated beaches to refer to as proof, the evidence of this fails us. The glacier had probably exterminated the temperate-climate life also of the seas just south of Long Island; for numerous bowlders, from one ton to two thousand tons in weight, of New England origin, show that the ice stretched on from the Sound in full force over Long Island, and must have had its terminal ice-cliffs somewhere to the south in the shallow water of the

* Prof. Mather in his New York Geological Report (p. 262) gives, from the verbal reports of others, statements respecting the discovery of shells on Long Island, which are, in part at least, of uncertain credibility, and are supposed by him to indicate, where of value, the presence of Tertiary deposits or of Tertiary and upper Cretaceous, beneath the Drift. Besides the localities on Long Island, at depths varying form thirty to more than a hundred feet, shells are stated to have been obtained on Governor's Island, in New York Harbor, at a depth of about 100 feet. None of the shells or localities have ever been seen by a geologist, so that the precise value of the evidence is still in doubt.

Atlantic border, if not along the southern shores of this island. Under these circumstances the old life, which might early have repeopled the southern shores of Long Island and the seas on the east, would have been slow in repossessing itself of Long Island Sound.

Although the Sound has yet afforded no evidence of molluscan life in the Champlain period, there is a deposit full of shells, according to Mr. Sanderson Smith,* on Gardiner's Island, off the east end of Long Island at the entrance to Peconic Bay. The bed has a height of fifteen or twenty feet above the sea level. The species mentioned in the article are: *Nassa trivittata*, *N. vibex* Say, *Fusus decemcostatus?* Say, *Purpura lapillus* Lam., *Columbellu lunata* Say, *Natica duplicata* Say, *Chemnitzia interrupta* Stimp., *Crepidula unguiformis* Lam., *C. fornicata* Lam, *Bulla canaliculata* Gould, *Ostrea borealis* Lam., *Pecten Islandicus* Chemn., *P. Magellanicus* Lam., *Arca transversa* Say, *Cardita borealis* Conrad, *Astarte sulcata* Fleming, *Venus mercenaria* (very thick and heavy shells, broken but not rolled and making the bulk of the deposit), *Mactra lateralis* Say, *Mya arenaria* Linn., besides three other doubtful species.

In the above remarks I have assumed that the ice which carried the bowlders to Long Island was that of the glacier and not of detached icebergs; in other words, that the glacier did not terminate along the Connecticut shores in the early part of the Champlain era during the melting, and give off icebergs for farther southward transportation. This point, which has some arguments in its favor, I propose to consider in a future paper on the geology of Long Island. Even if the glacier had its southern limit along Southern New England, the Sound would have been filled with the floating masses, and its waters, greatly freshened by the under-glacier streams, poured in along the whole coast and by the melting of icebergs; the exclusion, therefore, of the molluscan, articulate and vertebrate life of existing species from the Connecticut coast would have been nearly or quite complete.

ART. XXXVIII.—*Corals at the Galapagos Islands*, by L. F. POURTALÉS.

THE Galapagos Islands are, as is well known, an important point in the geographical distribution of corals, being almost exactly on the boundary of the coral-producing part of the Pacific Ocean, and that portion which is destitute of them on account of the low temperature of the water. All the writers

* Ann. Lyc. Nat. Hist. of New York, viii, 149, May, 1865.

on the subject have placed this group of islands in this latter portion. During the visit of the United States Coast Survey steamer Hassler, a number of specimens of corals, of which the following is the list, were picked up on the beaches of several of the islands:

Ulangia Bradleyi Verrill, Indefatigable Island.
Pavonia gigantea Verrill, James Island.
Pavonia clivosa Verrill, Indefatigable Island.
Pavonia, sp., James Island.
Astropsammia Pedersenii Verrill.
Pocillipora capitata Verrill, Jervis and Charles Islands.
Porites, sp.

The undetermined *Pavonia* is a massive species with larger calicles than those of the two other ones, and more porous and lighter. The specimen is too much rolled for nearer determination. The *Porites* is massive, also, and in the same condition.

The species are all, or nearly all, identical with those found at Panama. They are mostly reef-builders, but here live probably isolated and at a certain depth, having never been observed *in situ*. In individual growth they are fully equal to those from more favored localities, the rolled pieces of *Pavonia* measuring six or seven inches in diameter, thus indicating masses of considerable size originally. They are not confined to the northernmost islands of the group, where we should more naturally look for them, from the greater proximity to the warm current, but as the list shows, a *Pocillipora* was found at Charles Island, one of the southernmost. The probability of fragments drifting from one island to the other is very small, owing to the considerable depth of water between them.

ART. XXXIX.—*A Comparison between the Ohio and West Virginia sides of the Alleghany Coal-field;* by E. B. ANDREWS.

[Read before the American Association, at the meeting at Detroit.]

IN the study of the Alleghany coal-field it is necessary to have some well defined geological horizon to serve as a datum line with which to collate the various strata. Sometimes the underlying formation—conglomerate in western Pennsylvania and Waverly in Ohio—is taken for such base line and measurements are made to the strata above and the coal-seams are lettered or numbered accordingly. But the conglomerate is an uncertain formation and the Waverly presents a very undulating and uneven surface, as shown by Mr. M. C. Read and myself in the Ohio Geological Reports. In this discussion, I

prefer to take the horizon of the Pittsburgh seam of coal as the base of measurement.

This seam is of wide extent, being 225 miles long and about 100 miles wide, and is easily recognized by geologists. It has been asserted that this seam was formed in a trough with the Alleghany mountains forming a sloping side on the one hand and the Cincinnati uplift on the other, and as the subsidence—which is conceded to have been regular and uniform—continued, the marsh, with its accumulated vegetable matter, crept up either slope. In time the center or lowest part was buried by sediments and new land surfaces were formed on which vegetation grew and other resulting seams of coal stretched across the trough from side to side like the chords of an arc, but all coalescing with the great seam ever rising along the margins to meet them. I know of no proof whatever that the Alleghany mountains were uplifted just before the era of the upper coal-measures, nor is there any that the slope on the side of the Cincinnati uplift differed at the time the Pittsburgh seam was formed from what it had been during the era of the lower coal-measures. There is, furthermore, no proof that the upper and lower coal-measures are unconformable, as they must necessarily be if the theory controverted be true. Moreover, as a matter of fact, in Ohio, in locations which I have carefully examined, where the seams above the Pittsburgh seam are said to coalesce with the latter, there is no such coalescing, but the seams continue westward long distances beyond the points where the union is supposed to have taken place.

Disregarding, therefore, the discredit attempted to be thrown upon the great Pittsburgh seam as exceptional in its mode of formation, I shall confidently assume it as a trustworthy datum line to guide us in our investigations.

This seam occupies nearly the center of the northern and wider part of the Alleghany coal-field and extends through portions of Pennsylvania, Ohio, and West Virginia. It does not reach Kentucky, its limit of extension to the southwest being not far from the mouth of the Guyandotte river in West Virginia.

Now, if we take this seam of coal and follow around its line of outcrop and measure from it down to the base of the productive coal-measures, we find the intervals quite uniform through the larger part of the circuit. In Ohio, according to Dr. Newberry's measurements and my own, it is from 700 to 800 feet. In Pennsylvania on the Alleghany river north of Pittsburgh it is reported by Prof. H. D. Rogers to be from 600 to 700 feet; or 800 feet including the conglomerate underneath. In the northern part of West Virginia, a little south of the Pennsylvania line, it is reported to be 500 feet. But in West

Virginia farther south, the interval is far greater, as will be shown. If we make a section across the whole coal-field, including the Ohio side of the synclinal axis, taking the general line of the Kanawha river, we find the southeastern side of that axis not only very much wider territorially, but also containing a much greater thickness or depth of productive coal-measures. The Pomeroy seam of coal, which I have shown in the Ohio Geological Reports to be identical with the Pittsburgh seam, dips to the southeast under the Kanawha river a little above its mouth. A few miles higher up the river, it reappears on the other side of the synclinal axis and has been identified by Prof. W. B. Rogers and others. From this point we descend the geological series in going to Charleston, or perhaps to a point a little above Charleston, through an interval estimated to be 700 feet. Professor Fontaine, who has contributed some valuable articles to the American Journal of Science on the Geology of West Virginia, writes me that he thinks it may be greater than this. From Charleston to the Kanawha falls we descend in the series from 1,100 to 1,200 feet. I measured the interval between the coarse conglomeratic sandrock of the falls and one of the upper coal-seams on Cotton mountain, a seam which is believed to dip below the river a little above Charleston, and found it nearly 1,200 feet. Prof. Fontaine estimates 1,200 feet as the thickness of the coal-measures between the top of the conglomeratic sandrock of the falls and the southeastern outcrop of the measures up New river. A thousand feet have been measured and he estimates 200 feet for a space not measured. This makes a total of 3,100 feet from the horizon of the Pittsburgh seam to the base of the Productive measures. This is exclusive of 2,132 feet of lower shales and limestones; the Lewisburg limestone, according to Prof. Rogers, being 822 feet thick. This limestone is perhaps local, as Prof. J. P. Lesley makes no mention of finding it in his investigations in Tazewell, Russell and Wise Counties, to the southwest, where he found the lower coals and underlying Devonian rocks caught in curious lines of faults and upthrows which have brought to the surface lower Silurian limestones. But the Lower Carboniferous limestones appear in full force farther southwest in Tennessee. We have, therefore, about 2,400 feet more depth of Productive coal-measures below the Pittsburgh seam in West Virginia than in Ohio and Western Pennsylvania. My own personal explorations on the tributaries of the Kanawha, and on the upper waters of the Guyandotte and of the Tug fork of Big Sandy lead me to the belief in a corresponding thickness of coal-measures in all that portion of the State extending southwestward into Kentucky.

Perhaps the most valuable range of bituminous coals in the United States is that belt, which, beginning upon Elk river,

passes the Kanawha between Charleston and Kanawha falls, includes the Coal river field, and crosses the Guyandotte, the upper Twelve Pole, and Tug and Louisa forks of Big Sandy. This belt belongs geologically to a space in the vertical series, which is below the horizon of the base of the coal-measures in Ohio and Western Pennsylvania. Yet below this group comes in another, which Prof. Fontaine designates as the Conglomerate series. If we replace the strata, now inclined on either side of the synclinal axis, in their original horizontal position, we find the proof of a vast depression of the surface in this portion of West Virginia at the beginning of the series of Productive coal-measures. It was doubtless a trough, a geosynclinal, according to Prof. Dana's nomenclature, and a part of the great Appalachian system of the wrinkling and folding of the continent. To the southeast there were at the time elevated lands of older formations, stretching along the same Appalachian line of disturbance and upheaval.

On the northern and western side of this vast basin lay the marginal plateau of the Waverly more than 2,000 feet high, with the yet higher lands of the Cincinnati uplift in the distant western background. This trough had somewhere a connection with the ocean and gradually sank below the water, while at the same time the rivers from the highlands, more or less remote, brought in sediments of various kinds. The muds and sands sometimes accumulated so as to be above the water and, during intervals of repose in the subsidence, vegetation crept out over the newly formed surfaces and formed great marshes, which, when subsequently buried, gave to mankind seams of coal. The spaces between the seams of coal were at first chiefly filled with sand which now constitutes the massive sandrocks which characterize the whole region of the lower coals of West Virginia. Sometimes the sands were coarse and gravelly and formed true conglomerates. The conditions favorable to the formation of limestone did not exist, and in the lower 2,000 feet of accumulations, I have seen, in all my extended explorations of the region, only a single limestone layer, and that in Webster County, on the head of Elk river. I had no opportunity to examine it for fossils.

When at last, in the subsidence, this basin was filled, the waters came over the marginal plateau to the north and west, a region remarkable for its stability and for the uniformity and evenness of its subsidence. At first, over the newly submerged space was spread a more or less continuous sheet of coarse sand and gravel which now constitutes the coal-measure conglomerate of Ohio and Western Pennsylvania. The continuity of the sheet is often broken and large areas around the margin of our Ohio coal-field show no conglomerate whatever. The first

coal marshes grew in the hollows and depressions of the Waverly, depressions formed, it may be, wholly or in part, by erosions from surface drainage during the long period in which the formation had remained high and dry land. After the whole plateau had been submerged the same process of filling up, and marsh-forming and coal-making, went on, as has already been described, with the exception that the waters were more habitable and the layers of fossiliferous limestone show the remains of crinoid, mullusk and fish.

Less sand and more mud was brought in and shales abound, although sandstones are not uncommon, even those that are conglomeratic in character. The Pittsburgh seam of coal, which I have used as my basis of measurement, was by no means the last in the upward series. There are above it in West Virginia, probably 1,200 to 1,500 feet of coal-measure rocks, with several seams of coal. This would give from 4,300 to 4,500 feet of productive coal-measures in that favored State. No other State in the Union contains so great a vertical range.

This great Kanawha basin or trough originally extended to the south or southwest so as to include the area in which the coals of Montgomery county, Va., were formed, for these coals are doubtless the equivalents of the lowest on New river, as are also the remnants and outliers of coal in Tazewell, Russell, and Scott counties.

The subsequent upheavals and erosions have thrown great difficulties in the way of determining the course and extent of this ancient trough. I have no sections along the southeastern side of the coal-field in Kentucky—for these we must look to Prof. Shaler, now engaged in the prosecution of the Kentucky Survey; but in northern Tennessee, on the same side of the field, Prof. Safford reports about 2,500 feet of coal-measures above the horizon of the Mountain limestone. It is impossible, in the absence of sections through Kentucky, to determine whether these 2,500 feet in Tennessee were formed in a trough of equal depth with that on the Kanawha. It was doubtless approximately so, and we have here a clue to the direction of the trough to the southwest. If the Lewisburg, West Virginia, limestone is the equivalent of the Mountain limestone of Tennessee, we may infer that this formation, wherever it existed along the line of the geosynclinal depression, was carried down, while the lower Carboniferous limestone of Ohio and Northeastern Kentucky, on the northwestern side of the synclinal axis, were not involved in the movement. In southern Tennessee, Prof. Safford reports in one of his sections only about 800 feet of coal-measures above the Mountain limestone, the upper 1,700 feet having doubtless been removed by denudation. If not thus removed, the limestone was not carried

down in the geosynclinal, but remained on the higher margin as in Ohio, and the original trough by which connection was had with the sea lay probably to the east.

Did the same geosynclinal trough extend in the opposite direction from the Kanawha valley, i. e., to the northeast through Pennsylvania? If the equivalency of the Pittsburgh seam has been rightly determined in the Cumberland and Broad Top coal-fields, we have in the former, according to Mr. Tyson, 1,100 feet of productive measures below that seam, and in the latter about 700, according to Prof. Lesley. Prof. Lesley has recently reported finding in the Broad Top field thin coals in the lower Carboniferous, in strata which he regards as equivalents of the Waverly Berea grit of Ohio. This is a discovery of the highest interest, but it will not effect this discussion, since I confine myself to what we term the Productive coal-measures. The geological interval between the middle of the Waverly, with the Chester limestone above, and these measures is a very great one. It may be remarked in this connection that there are doubtless areas where the lowest portions of these measures contain no register of themselves in seams of coal, the conditions of coal-making not existing, but we have so little definite information on this point that it appears to be necessary to make our limit of depth the lowest known coals of the proper coal-measures. It is quite possible, for example, that the shales, &c., between the lowest coal-seam on New River, West Virginia, and the Lewisburg limestone may somewhere contain seams of coal, but it is not best in this paper to assume the existence of such facts.

In the anthracite coal fields of Pennsylvania there is so much uncertainty as to an equivalent of the Pittsburgh seam that I fear we shall gain little or nothing from them to bear upon the question of the original depth of the basin in which they were formed. Prof. Lesquereux, in his chart of grouped sections in the fourth volume of the Kentucky Survey, places all the coal-seams of Wilkesbarre, Pittston, Scranton and Carbondale below the horizon of the Pittsburgh seam. In the Pottsville region Mr. Daddow claims that seam G. of his series, is the equivalent of the Pittsburgh seam. The seam G. is 650 feet above the lowest seam in the series, one imbedded in conglomerate. If we accept this equivalency we find no proof of the existence of the deep geosynclinal trough in that direction. If, again, the conglomerate under the anthracite basins is the equivalent of the conglomerate of Western Pennsylvania and Ohio, and they were once one and continuous as the Pennsylvania geologists affirm, then, since to the west, the conglomerate was not deposited until after the great West Virginia trough was filled and the waters in the general subsidence

came over the great marginal plateau already described, we may well infer that there could not have been an extension through the anthracite region of the geosynclinal valley. There was only enough of greater depth to include the increased thickness of the conglomerate.

The comparison of the two sides of the Alleghany coal-field suggests a very great difficulty in regard to the determination of the place of what is termed the coal-measure Conglomerate. Is there an established horizon for such Conglomerate in American geology? In Ohio we now are in the habit of calling that rock the Conglomerate which, when found at all, lies upon the Waverly (although formerly the Waverly conglomerate was also thus designated) and is approximately from 600 to 800 feet below the horizon of the Pittsburgh seam of coal. The same Conglomerate extends under the bituminous coal-field of Western Pennsylvania and holds the same relation to the Pittsburgh coal. This conglomerate horizon should have the same relative distance below the Pittsburgh seam in West Virginia, and I think I have probably found lithological proof of it in a very coarse conglomerate on Elk River—a branch of the Kanawha which enters the latter at Charleston. At the Kanawha Falls is a conglomerate more than 1000 feet lower in the series. This rock has been honored with a capital C, and the coals in the 1200 feet below it are spoken of as the Conglomerate series. Still below this series and below all the coals, Prof. W. B. Rogers finds another conglomerate in the New River valley as does Prof. Lesley under the lowest coals in the faults and upthrows to the southwest. Now if the coal-measure Conglomerate is a geological horizon and not merely a rock, these various strata cannot all be the true Conglomerate. We find conglomerates everywhere in the vertical range of the coal-measures. They are found even above the horizon of the Pittsburgh seam. Heavy conglomerates abound along the western side of the eastern Kentucky coal-field, at various elevations above the lower Carboniferous limestone, and the same is true in Tennessee.

In the Indiana and Illinois coal-field, the coal seams of which have never been synchronized with those of the Alleghany coal-field, we have reported a conglomerate or millstone grit, but no one knows its exact place in the great time scale, or whether it is the equivalent of any of the half dozen well-marked conglomerates of the Alleghany field. So far as we now know it may have no equivalency any where.

In Arkansas there is a conglomerate called in the Geological Reports the Millstone grit. The available coals of the State are said to lie below it, and are therefore called the Sub-Conglomerate coals, although Prof. Lesquereux asserts that they

belong to the true coal-formation and cannot be separated from it. The conglomerate in this case can have nothing more than a lithological significance.

In the South Joggins coal-field at the head of the Bay of Fundy, Sir Wm. Logan reports 14,570 feet of coal-measures. In this vast series of strata, and not very far from the middle of it, Dr. Dawson, in his admirable work on Acadian Geology, finds a group of coarse sandrocks which he denominates the Millstone Grit series. Here the millstone grit is not known to be the equivalent of the millstone grit of Great Britain, nor of any of our conglomerates of the United States.

In view of this confusion, I think all will agree with me that the terms *millstone grit* and *coal-measure conglomerate* should either be used to designate a uniform horizon in the Carboniferous system or be abandoned as geological terms and retained only for their lithological meaning.

I close this paper with another remark by way of inference, viz: that there is need of an entire revision of the classification of the coal-seams of the great Alleghany coal-field. It seems strange to begin in the middle of the vertical series and call a seam of coal No. 1, or letter A, and enumerate upward, while all the seams below are left without enumeration. But the data for such revision are not yet sufficient.

Art. XL.—*Instinct? in Hermit Crabs;* by Alexander Agassiz.

While tracing the development of one of our species of Hermit Crabs I raised from very young stages a number of specimens till they reached the size when they need the protection of a shell for their further development. I was of course curious to see how they would act the first time when supplied with the necessary shells. For this purpose, a number of shells, some of them empty, others with the animal living, were placed in the glass dish with the young crabs. Scarcely had the shells reached the bottom before the crabs made a rush for the shells, turned them round and round, carefully examining them, invariably at the mouth, and soon a couple of the crabs decided to venture in, which they did with remarkable alacrity; and after stretching backward and forward, they settled down into their shell with immense satisfaction. The crabs who were so unfortunate as to obtain for their share living shells, remained riding round upon the mouth of their future dwelling and on the death of the mollusk, which generally occurred soon after in captivity, commenced at once to tear out the animal, and having eaten him, proceeded to take its place within the shell.

It is of course very difficult to apply to Invertebrates many of the laws of natural selection, and thus far we know so little of the habits of most of our marine animals that it is idle to speculate upon the effect of causes which may effectually modify the life of higher animals. In the case above mentioned there is no possible connection between the embryo and the parent to account for the young having learned from the former the use of the shell and its value for his existence. We can, therefore, only explain the faculty of performing this act as inherited, or else as a simple mechanical act rendered necessary by the conditions of the young hermit crab. The latter seems the more probable case from the nature of the test of the hermit crab in its younger stages. While the young hermit crab soon after leaving the egg is still provided with its powerful temporary swimming feet, and while the feet of the adult can only be traced as mere rudiments behind them, the whole test of the cephalothorax and abdomen (which are symmetrical) is of considerable consistency up to the last moults preceding the stage when it seeks a shell. At that time the young are no longer symmetrical, the feet, which are now fully developed being largest on the right side and the abdomen, beginning to curve in the same direction away from the longitudinal axis. When the moult has taken place which brings them to the stage at which they need a shell, we find important changes in the two hind pairs of feet, now changed to shorter feet capable of propelling the crab in and out of the shell; we find also that all the abdominal appendages except those of the last joint are lost, but the great distinction between this stage and the one preceding it is the curling of the abdomen; its rings, so distinctly marked in the previous stages are quite indistinct and the test covering it is reduced to a mere film, so that the whole abdomen becomes of course very sensitive. It is therefore natural that the young crab should seek some shelter for this exposed portion of his body, and from what I have observed, any cavity will answer the purpose; one of the young crabs having established himself most comfortably in the anterior part of the cast skin of a small isopod, which seemed to satisfy him as well as a shell, there being several empty shells at his disposal. This mechanical explanation still leaves unanswered the eagerness with which the crabs rushed for the shells, their careful examination of its openings, their taking the animal out and occupying his place; all acts which seem to require considerable intelligence?, and to show remarkable forethought?.

Newport, Aug. 23, 1875.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Constitution of Ammonium Compounds.*—In their previous paper,* Meyer and Lecco established the identity of the two di-methyl di-ethyl derivatives of ammonium, whether obtained from di-methylamine and ethyl iodide on the one hand, or from di-ethylamine and methyl iodide on the other, by the complete equality of their physical properties, and of their chemical reactions in solution, by the identity of their decomposition-products at high temperatures, the equal solubility of their platinum salts, which were identical in crystalline form, and by the exact similarity of fusing point of their picrates. To these particulars, they now add the crystallographic identity of their picrates as determined by Groth and Arzruni. Lossen having objected that the conclusion drawn from the above facts—i. e., that nitrogen was a pentad in these compounds—was not warranted unless it could be shown that no exchange of radicals took place within the molecule, the authors have made a series of experiments to test this question. By acting on tetra-methylammonium iodide with ethyl iodide, there would result, if an exchange did actually take place, the following reaction:

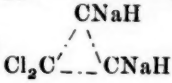
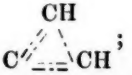
$$N(CH_3)_4I+C_2H_5I=N(CH_3)_3C_2H_5I+CH_3I.$$

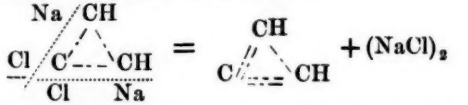
The two substances were heated together in a sealed tube, first to 100° and then to 150° (the original reaction having been completed at 80°); but no trace of any reaction could be detected. The same result was obtained when methyl iodide was heated with tetrethylammonium iodide to 70°, to 150, and even to 180° (the previous experiments having been conducted at ordinary temperatures). The authors hence reiterate their conclusion that the ammonium derivatives are not molecular but atomic compounds and that in them, the nitrogen is quinquivalent.—*Ber. Berl. Chem. Ges.*, viii, 936, July, 1875. G. F. B.

2. *On Retene.*—Among the products obtained in the destructive distillation of wood, is a heavy oil often employed in lubrication. This oil is very rich in the hydrocarbon retene, and Ekstrand has extracted this substance from it and submitted it to an investigation. On pressing the cooled oil, a grayish soapy mass is obtained having the odor of creosote and melting at 84°–85°. After washing with ether, solution in boiling alcohol, decolorization with animal charcoal and crystallization, pure retene is obtained in white micaceous scales, fusing at 98°·5 and having the composition $C_{18}H_{18}$. Crystallized retene has a density of 1·13, and fused of 1·08. It is readily soluble in boiling alcohol and glacial acetic acid, and in carbon disulphide, benzene, and ether. It unites directly with chlorine and bromine at ordinary temperatures and is readily oxidized by nitric acid. The picrate crystallizes

* This Journal for June, 1875, p. 462.

in orange needles. Di- and tetra-bromo-retene, and a sulpho-conjugated acid and its salts, are described. In acetic acid solution, chromic acid oxidizes it to dioxyretistene $C_{16}H_{14}O_2$ and to two other bodies, $C_{16}H_{16}O_3$ and $C_{18}H_{17}O_2$, both monobasic acids, forming well defined salts. Dioxyretistene reduced by zinc powder, yields dibenzyl. Hence he regards the retistene of Wahlforss as probably only a mixture of retene and dibenzyl.—*Bull. Soc. Ch.*, II, xxiv, 55, July, 1875. G. F. B.

3. *On Propargylene, a new hydrocarbon, C_3H_2.*—By the action of strong bases on croton-chloral, dichlorallylene, $C_3H_2Cl_2$ is obtained. PINNER has now observed that sodium is capable of uniting directly with this body, and that the product when decomposed by water, yields a gas having the composition C_3H_2, which he calls propargylene. If fragments of sodium be placed in dichlorallylene at ordinary temperatures, a slight almost imperceptible evolution of gas takes place, but in the course of half an hour, the sodium sinks to the bottom, having been converted into a brown dull mass, with a notable increase of volume. Finally, when somewhat less than a molecule of sodium has been added to one of dichlorallylene, the latter has entirely disappeared, the dry mass being the sole product. Water, alcohol, and ether decompose it, evolving torrents of gas and forming sodium chloride. It has not been obtained pure and its analyses are discordant; but its mode of formation gives it the probable formula $C_3H_2Cl_2Na_2$. If it be treated with water and the evolved gas be passed through bromine, this is absorbed, and a heavy oil is obtained, having the formula $C_3H_2Br_4$. By the action of potassium hydrate upon this, tribromallylene C_3HBr_3 is obtained and this unites directly with Br_2 to give C_3HBr_5 in white crystals. Propargylene gas passed through ammoniacal silver solution, gives a precipitate of fine white needles, which rapidly blacken even in the dark, and have the probable composition $(C_3H_2)_2Ag_2O$. Pinner regards the sodium compound as composed thus:  , and propargylene thus:  ; and the decomposition of the former by water is represented as follows:



Hence this dichlorallylene is not a derivative of allylene proper, but is propargylene dichloride.—*Ber. Berl. Chem. Ges.*, viii, 898, July, 1875. G. F. B.

4. *Synthesis of a dextro-rotatory Malic acid.*—BREMER has published a preliminary note on a new malic acid which rotates the polarized ray to the right, produced from dextro-rotatory tar-

taric acid. Ordinary tartaric acid, iodine, phosphorus and a little water, were enclosed in sealed tubes and heated in a water-bath for several days. The mass was treated with water, the succinic acid crystallized out, the mother liquor diluted and the phosphoric and unattacked tartaric acid separated by milk of lime. Ammonium nitrate was added and the solution evaporated. On the addition of alcohol, calcium malate and succinate were deposited. They were converted into lead salts, the lead separated by H_2S, and the acids obtained by evaporation, were separated by the deliquescence of the former. Recrystallization of the acid ammonium salt afforded a pure malate, having the same form as the natural salt and fusing at 170° C. Examined with the Polaristrobometer of Wild, the salt was found to rotate to the right. The author is now engaged in the attempt to produce a left-handed malic acid from lævotartaric acid, in the hope that from the two an inactive malic acid may result, as racemic comes from the two tartaric acids.—*Ber. Berl. Chem. Ges.*, viii, 861, July, 1875. G. F. B.

5. *On Ethyl and Amyl-sulphocarbonates as remedies for the Phylloxera.*—Dumas, having shown that potassium sulphocarbonate, when mixed with the soil, evolves hydrogen sulphide and carbon disulphide, recommended this substance for destroying that pest of the grape culture, the phylloxera, since it was well known that carbon disulphide was rapidly fatal to them. Moreover, he showed that the hydrogen sulphide evolved simultaneously, did not, under these circumstances, seriously injure the grape plants, being rapidly oxidized in the soil. Experiments made by Zöller and Grete at the Agricultural High School of Vienna, have confirmed the excellence of potassium sulphocarbonate for this purpose; but they have also shown the equal usefulness of another compound, which, while at the same time evolving in the soil the noxious carbon disulphide, does not set free the injurious hydrogen sulphide. Moreover, while the former substance recommended by Dumas is difficult to prepare and is consequently high in price, the new compound can be made with facility and at a moderate price. This new body is potassium ethylsulphocarbonate or xanthate. A solution of this salt mixed with the soil, evolves CS_2 copiously. It is however better to mix the dry salt with a small quantity of earth and of superphosphate, and to scatter the dry powder about (or better directly upon) the roots. In this way, both the potash and the phosphoric acid become useful finally to the plant.

In a subsequent paper, the same authors give the results of some experiments on the action of the xanthate on plants. They found that even tender succulent plants growing in only half a liter of earth, could be treated with a gram of the above salt, with no injurious effect other than the loss of a few leaves. Shrubs were entirely unaffected when treated with from 3 to 5 grams. They also reply to Dumas's statement that the xanthate is too costly in France to be used practically, owing to the high price of alcohol, supposing the fused potassium hydrate out of the question. They

would replace the ethyl by amyl, forming thus the amylsulphocarbonate. This they find by experiment is equally efficacious with the other, has even less injurious action on plants and is far cheaper. Moreover, it may be made easier. If concentrated potassium hydrate solution be agitated with crude amyl alcohol, and carbon disulphide be added, the whole mass becomes warm and deposits potassium amylsulphocarbonate, in a form best suited for use. It may be employed either in solution or better mixed with superphosphate. As to cost, the authors estimate that at the present cost of amyl alcohol (fusel oil) in Vienna, it can be made for $15 a hundred weight. It is not only useful to exterminate the phylloxera, but also many other root parasites of plants.—*Ber. Berl. Chem. Ges.*, viii, 802, 955, June, July, 1875.

G. F. B.

6. *On the occurrence of Ethyl Alcohol in Plants.*—The occurrence of ethyl alcohol in the unfermented juices of plants is rendered probable by the fact that these juices contain not only acetic acid, its oxidation product, but also its homologues, methyl, hexyl, and octyl alcohols. GUTZEIT has made, in Geuther's laboratory, a somewhat extended investigation of this subject, using for the purpose *Heracleum giganteum*, *Pastinaca sativa*, and *Anthriscus cerefolium*. Six and a quarter kilograms of the fruit of the first plant being distilled with 18 kilograms of water, gave 12 kilos. of a distillate, from which the oil on the surface was separated by means of a syphon. The residue on distillation gave 12 to 15 grams of volatile liquids, which after rectification boiled between 72° and 77°, and consisted of methyl and ethyl alcohols, the latter consituting two thirds. By examining the fruit at various stages of growth, the author infers that as ripening approaches, the ethyl compounds gradually diminish, being converted into more condensed bodies; while the methyl compounds remain constant. The other plants mentioned gave a similar result.—*Liebig's Annalen*, clxxvii, 344, June, 1875.

G. F. B.

7. *On Arbutin.*—HLASIWETZ and HABERMANN have made a research upon arbutin, a glucoside extracted by Kawalier from the *Arctostaphylos uva-ursi* or bearberry. Its discoverer observed that it was easily split into glucose and a body which he called arctuvin, but which Strecker asserted to be hydroquinone. The authors find, however, that the body thus obtained is not pure hydroquinone, but is a mixture of this and its methyl derivative, methyl-hydroquinone $C_6H_4\begin{cases}OCH_3\\OH\end{cases}$ isomeric with saligenin $C_6H_4\begin{cases}CH_2OH\\OH\end{cases}$. Hence they assign to arbutin the formula $C_{25}H_{34}O_{14}$ and express its splitting by ferments, as follows:

$$\underset{\text{Arbutin.}}{C_{25}H_{34}O_{14}} + \underset{\text{Water.}}{(H_2O)_2} = \underset{\text{Hydroquinone.}}{C_6H_6O_2} + \underset{\text{Methyl-hydroquinone.}}{C_7H_8O_2} + \underset{\text{Glucose.}}{(C_9H_{12}O_6)_2}$$

One hundred parts of arbutin yield 19·7 hydroquinone, 22·5 methyl-hydroquinone and 64·7 sugar.—*Liebig's Annalen*, clxxvii, 334, June, 1875.

G. F. B.

8. *A new Base obtained in the Aniline manufacture.*—JACKSON has examined in Hofmann's laboratory a new base obtained from the last run of the still in the manufacture of aniline. This product—a black tarry liquid with a disagreeable odor—was dissolved in hydrochloric acid, diluted and filtered. Upon addition of sodium hydrate, the bases were set free as a light oil swimming on the surface. This was fractionated, and the portion boiling between 280° and 320°, separated from naphtylamine by sulphuric acid, gave a black oil, which converted into the nitrate and purified, was proved to have the composition $C_{13}H_{13}N$ or $C_{13}H_{11}NH_2$, a primary amine. Its nitrate, sulphate, and chloride crystallize well. Farther investigation showed it probably to be tolyl-phenyl-amine, $(C_7H_6.C_6H_5.)NH_2$.—*Ber. Berl. Chem. Ges.*, viii, 968, July, 1875. G. F. B.

9. *Viscosity of Saline Solutions.*—M. A. SPRUNG has repeated the experiments of Poiseuille to determine the relation of the viscosity of liquids to their temperature and chemical composition. A volume of 21·15 cms.^{3} of the liquid was forced through a capillary tube 300 mms. long, and ·46 mms. in diameter, under a pressure of 158 cms. of water. Observations were made with solutions of chloride of ammonium of five different strengths, at intervals of temperature of 5°, from 0° to 60°. A graphical representation shows that the curve of viscosity of the liquid cuts that of water at a temperature as much greater as the solution is stronger, and that at low temperatures the viscosity is less, at high temperatures greater, than that of water. The same result was shown by eight other salts.

By varying the acid it appears that salts of potash, soda and ammonia arrange themselves in the following order with the acids: H_2SO_4, HCl, HNO_3, $HClO_3$, HB_2 and HI. In each of the three series the viscosity diminishes from sulphuric acid to iodhydric acid. The acids remaining the same, these salts stand in the order NaOH, KOH and NH_4OH, in which the soda salts flow most slowly and the ammonia salts most rapidly. Sulphate of ammonia, however, flows more slowly than sulphate of potash.

If we compare the velocity of flow, of salts of similar constitution, we find that at all temperatures the viscosity of the solution is as much greater with the same base as the molecular weight of the acid is less, and with the same acid as the molecular weight of the base is less.

The viscosity of the same solution of sulphate of oxide of chromium is different according as the salt belongs to the violet or green modification, being greater in the first case than in the second.—*Bib. Univ.*, ccx, 112. E. C. P.

10. *Photographic Irradiation*.—Capt. ABNEY states that the commonly received view, that the increase in the apparent size of the image of a luminous object when photographed, is due to simple reflection from the back of the plate, is quite untenable. If examined under a microscope, it will be seen that the film consists of minute particles separated by considerable distances from one

another. Their diameter is several times the length of a wave of light, and hence we may treat the question as a case of simple geometrical reflection. Computing by Fresnel's formula the amount of light reflected by the collodion and glass at various angles, we find the maximum amount of light would fall at a distance $2t \cot v$, in which t is the thickness of the glass, and v the critical angle. On development, therefore, a ring might be formed around the image with this distance as a radius, and shading off more abruptly inside than outside. To test these results an image of the sun was formed by a lens of 6″ focus on a plate ·12″ thick. A well defined annulus was formed shaded as described above. With a plate of double thickness, the mean diameter of the annulus was doubled. Two plates were then employed separated by a thin card. A ring was formed on the first plate and a diffused image on the second. A measurement of the intensities agreed closely with the result of theory. Holes and slits were cut in platinum foil, placed on a dry plate and exposed directly to sunlight. Rings were obtained in one case, and parallel lines joined by semi-circles in the other, showing that the effect is not due to the lens of the camera.

Three methods suggest themselves for diminishing irradiation: first, to coat the back of the plate with some non-actinic color; secondly, to increase the opacity of the film; thirdly, to connect the particles of bromo-iodide of silver with a body of as nearly as possible the same density. The first is the usual method adopted by photographers in dry plate processes. This diminishes the irradiation from the back of the plate, but does not completely eliminate it, as will be understood when the nature of the resulting reflection is considered. The second method is easily carried out, and is more effective than the first. With albumen preservative particularly, the amount of irradiation is reduced materially, even though the film appears nearly transparent.—*Phil. Mag.*, l, 46. E. C. P.

11. *Conical Refraction.*—M. Nodot has endeavored to find a substitute for the crystal of arragonite commonly employed to show the phenomena of double refraction. The rarity of crystals of sufficient thickness of this substance, has led many physicists to improve the optical conditions so as to enlarge the angle of the exterior cone; but the field is still open, to find natural or artificial crystals capable of rivaling arragonite in specific refractive energy, thickness and transparency. Judging by the values of the three indices of numerous biaxial crystals, arragonite will be the most favorable mineral; but chemical substances are so numerous that we may hope to find crystals superior in these respects.

Three substances appear capable of replacing arragonite, sugar, bichromate of potassium and tartaric acid. The method of cutting the two first requires no trial, for a natural face of the sugur and a cleavage face of the bichromate are normal to one of the optical axes. For tartaric acid it is not the same, and the direction of

the faces through which we shall see one of the axes must be found by trial. We succeed easily by employing an Amici's microscope and are recompensed by the result, as crystals of equal thickness give cones twice as open as those of arragonite.—*Journ. Phys.*, iv, 166. E. C. P.

12. *Dielectric Capacity of Gases.*—M. L. BOLTZMANN has endeavored to determine this constant experimentally for gases, in order to determine its probable important relation to the index of refraction. According to the electromagnetic theory of light, of Maxwell, the square root of the dielectric capacity of a transparent medium should be equal to the index of refraction for rays of large wave-length. The capacity was determined by a condenser whose plates were separated by the gases to be tested, the potential being measured by a Kirchhoff's lecture electrometer, and a battery of 300 small Daniel cells being employed.

In the following table the first column gives the name of the gas, the second the square root of the dielectric constant, and the third the index of refraction:

Gas.	$\sqrt{k}$.	n.
Air	1·000295	1·000294
Carbonic acid	1·000473	1·000449
Hydrogen	1·000132	1·000138
Carbonic oxide	1·000345	1·000340
Nitrous oxide	1·000497	1·000503
Olefiant gas	1·000656	1·000678
Marsh gas	1·000472	1·000443

—*Pogg. Annal.*, clv, 403. E. C. P.

13. *The human body rendered luminous by Phosphuretted Hydrogen.*—Dr. GEO. MACLEAN of Princeton, in a private communication to the editors, says:

"Several years ago, after spending a portion of the day in experimenting with this gas (PH_3) prepared from phosphorus and solution of potash, on retiring to bed I found my body quite luminous, from a glow like that of phosphorus exposure to the air. Either some of the gas escaping combustion, or the product of its burning must have been absorbed into the system, and the phosphorus afterwards separated at the surface have there undergone eremecausis. I was conscious of no feeling that could be attributed to it, nor was my health apparently in any way affected by it."

Probably a repetition of the trial would reproduce the results observed by Dr. Maclean, which have apparently escaped notice heretofore because unsought.

II. GEOLOGY AND NATURAL HISTORY.

1. *Pseudomorphism and Metamorphism: a correction;* by JAMES D. DANA.—In my notice of Mr. Hunt's Essays in this Journal, volume ix (the number for Feb., 1875), I say, on page 109: "With the exception of the year 1858, I have never held

nor taught that "metamorphism is pseudomorphism on a broad scale." I would here correct the statement by dropping the exception, so as to make it read simply—

"I have never held that metamorphism is pseudomorphism on a broad scale."

When the sentence on page 109 was written I had the strongest personal conviction that I had never entertained this view as a broad general principle, notwithstanding the fact that the sentence stands in a book-notice of mine in this Journal for 1858. For, in 1871, when I first knew that the doctrine was attributed to me, I had no recollection of the sentence, or of passing through such a phase of belief. In addition, my manuscript lectures, which were written out in full for my geological course in 1854, and which were delivered in 1858, as they had been before and were subsequently, do not contain such a principle, any more than does my Manual of Geology, published in 1862, in which those lectures were embodied.

I can refer now to published evidence that the broad principle was never in my mind; and this evidence is afforded by the book-notice (xxv, 445, 1858) which contains the statement. I there—after expressing my confidence in Bischof's views respecting the "agency of alkaline silicates in pseudomorphism and metamorphism," and a reference to papers in volumes xlv, xlvii and xlviii (1843–1845) of this Journal, where I had advocated them—say as follows (the words in italics are those to which attention is directed):

"*On page* 92 *of the same paper* [that of vol. xlviii, 1845] metamorphism is spoken of as pseudomorphism on a broad scale."

The expression here cited certainly implies that the doctrine referred to had been taught by me as early as 1845. But on the page referred to there is no such general statement, in fact no general statement on the subject. Only the case of magnesian silicates is mentioned, besides that of silica;* and such silicates I have ever regarded as affording examples of pseudomorphism on a broad scale.† The introduction of an omitted word, so as to make the sentence read—

* The paragraph on page 92 is as follows:

"The importance of these discussions [on pseudomorphism] is not to be measured by the length of the catalogue of pseudomorphs. The same process that has altered a few crystals to quartz has distributed silex to fossils without number scattered through rocks of all ages. The same causes that have originated the steatitic scapolites occasionally picked out of the rocks have given magnesia to whole rock formations and altered throughout their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals altered from chrysolite, it is true also, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered, although often covering square leagues in extent. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents. The deposits of the so-called Rensselaerite [a variety of the magnesian mineral, talc] in Northern New York are other examples of the widely extended influence of the pseudomorphic agencies."

† The "extensive talcose formations" are now excepted, as they have been proved not to be magnesian or talcose.

"On page 92 of the same paper, metamorphism is spoken of as *sometimes* pseudomorphism on a broad scale"—
places it in harmony with my knowledge of my own scientific opinions and the statements in my various mineralogical and geological writings from the first to the last.

2. *On Triarthrus Beckii, supposed to have been found in a boulder in the Connecticut Valley;* by W. W. DODGE. (Letter to J. D. Dana, dated Oct. 22, 1873.)—Having seen last evening, in the October number of the American Journal of Science, the statement of your views there presented as to the age of the "Taconic" rocks, I communicate the following circumstance for what it is worth.

I saw recently in a friend's collection a cast labelled "*Triarthrus* —Connecticut Valley." The collector of the cabinet in which I found it died some years ago, and the present possessor can give no information as to the original. This may be from some well-known locality of whose existence I am ignorant, but as I saw at the same time numerous rock specimens labelled as from various towns in Western Massachusetts, I am inclined to think the cast is from a specimen owned by some friend to whom I have not access; and if really found in the Connecticut Valley, probably an erratic.

From what I have observed as to the distance at which boulders are usually found from the place where the rock was *in situ*, the presence of this fossil anywhere in the Connecticut Valley seems to me interesting.

3. *Forms and Origin of the Lead and Zinc Deposits of Southwestern Missouri;* by ADOLF SCHMIDT. (Trans. St. Louis Acad. Sci.)—The following is an abstract of this important paper:

These deposits are mainly in the Keokuk group (Subcarboniferous)—a group consisting of thin-bedded chert, or of limestone, or of chert and limestone interstratified, or of a calcareous chert called *silico-calcite.* The rocks are much altered and fissured and partly rendered magnesian, in the vicinity of the ores, and the cavities containing the ores appear to have been made when these alterations took place.

The lead and zinc ores of the "runs"—deposits of one horizontal direction not over 5,000 feet in height and width—are always associated with dolomite, and were evidently formed along fissures in a layer of limestone limited above and below by a chert layer, the rock adjoining which had been dolomized by "solutions of bicarbonate of magnesia;" the rock became cracked at the same time from the accompanying diminution of volume and made spaces for the ores. Crystals of galena isolated in the dolomite show that part at least of it was deposited in the rock "while the latter was in a magmatic condition."

In another kind of deposits called "openings," the ores occupy spaces which extend from a chert bed downward, but not through the limestone layer; they show no evidence of origin about vertical fissures, but rather from horizontal openings below a chert bed.

Again fissured chert beds are impregnated with lead and zinc ores—mostly sulphides—in the vicinity of the runs and openings, or else alone.

In still another variety of deposit, common in the Joplin district, galena occurs in loose accumulations of broken chert; and in part of Joplin creek valley the accumulations have a depth exceeding 70 feet. The ore appears to have been formed in the fissures of the chert beds, and, afterward, the chert beds were broken up through undermining by the removal of the limestone. The galena extends into the masses of chert and sometimes also is crystallized on their surfaces, as if its deposition had commenced before the breaking up of the chert but was continued during its progress.

There are also seams and crystals of galena and blende imbedded, or occupying pockets, in quartzyte, a gray to brownish and often bituminous variety. The quartzyte in such cases contains also irregular crystals of dolomite; and the cavities left by the removal of this dolomite are often filled with carbonate of lead or silicate of zinc.

These various deposits have evidently been very slowly formed through a long period of time, and the larger part after the partial dolomization of the beds along vertical and horizontal fissures, this making the spaces for the ores. There are no true veins. The layers of rock in which the ores occur lie only from 40 to 120 feet below the surface. Whether lead deposits exist below has not been ascertained. Exploration thus far is unfavorable to it.

4. *Carboniferous Conifers.*—Little attention seems as yet to have been given to the remains of Coniferous trees found in the Carboniferous rocks of the United States. In Nova Scotia several species are known, and are to some extent characteristic of definite horizons. In the Carboniferous sandstones of the United States such remains seem to be frequent; but I have seen no detailed account of them, and the only well characterized specimens which have come into my hands are portions of two trees from Ohio kindly sent to me by Dr. Newberry, and a very finely preserved fragment from Mazon, Grundy Co., Illinois. Both of these show the characteristic structure of *Dadoxylon* (Unger), *Araucaroxylon* of Schimper's recent work, and are closely allied to *D. materiarium*, the common species of the upper and middle coal formations of Nova Scotia. The specimen from Illinois is probably identical with that species. One of these from Ohio presents some peculiarities in the structure of the medullary rays, which may indicate a distinct species.

The subject is deserving of the attention of microscopists in the coal districts, as there can be little doubt that several interesting species remain to be discovered, and other kinds of structures; as for example the curious *Dictyoxylon* of Williamson, found also in Nova Scotia, would very possibly reward patient slicing of trunks showing structure.

The Devonian has also treasures of the same kind. In the

United States it has already afforded *Dadoxylon Halli* from New York, and *D. Newberryi* from Ohio, besides the curious *Ormoxylon Erianium*. No doubt other species remain to be discovered, especially in the Upper and Middle Devonian.

The writer of this note would willingly correspond with any one engaged in such researches, as he has now under examination a number of specimens from different parts of British America, and would be glad to have opportunities of comparison with those of the United States.

J. W. DAWSON.
McGill College, Montreal.

5. *Fossil Trees of Craigleith Quarry, Scotland.*—Six trunks of large trees have been obtained at Craigleith quarry since 1826. The largest, 36 feet long and 12 to 14 feet in girth, has been taken to the British Museum, and is to be set up erect there. Another is nearly 30 feet long and has been removed to the Botanic Garden. The trees were Conifers. The surface of each is bituminous coal, varying from one-twentieth of an inch in thickness to two inches. The trunks inside of this coaly exterior consist of carbonate of lime, carbonate of magnesia, carbonate of iron and free carbon in varying proportions. The coaly exterior is attributed, by Sir R. Christison, to bitumen passing to the surface as the destructive distillation of the wood was going on within.—*Proc. Roy. Soc., Edinburgh*, Jan., 1874.

6. *Volcanic History of Ireland;* by Prof. EDW. J. HULL (Roy. Soc., Ireland, vol. iv, part I).—Professor Hull describes as of *Lower Silurian* age, younger than Cambrian, sheets of "felstones" and porphyries of Wicklow, Wexford and Waterford, accompanied by beds of ash and volcanic breccia, interstratified with Lower Silurian slates and grits; also similar beds in the valley of the Boyne which become less frequent in the districts of Down, Armagh and Cavan. They are the representatives of the felstones and porphyries of North Wales, described by Sedgwick, Murchison and Ramsay, and those of Cumberland.

The Irish *Upper Silurian* contains igneous beds in the mountainous region of West Mayo and Galway; felstones, etc., along the western shores of Lough Mask; beds of ash, volcanic conglomerate and some of trap on the Promontory of Dingle, the thickness, according to Jukes, 2,500 feet, and interstratified with fossiliferous Wenlock beds.

The Irish *Devonian*, in the Killarney Mountains south of Lough Guitane, where a bold rock, 1490 feet high, a mass of columnar felstone, "indicates the position of an old 'neck' or volcanic throat, from either side of which, for considerable distances, beds of feldspathic ash and conglomerate, with large 'balls' of felstone, often hollow in the center, which were probably bombs shot out of the vent at various stages of the eruption." These beds of ash re-occur at Lough Garagarry, Lough Managh and the Devil's Punchbowl, and rise into the summit of Crohane (2,162 feet) and the southern slopes of Killeen Mountain. To the south, there is another district containing cotemporaneous trap rocks at the

mouth of Kenmare River. The shores of Lough Kay and Valentia Harbor are "another volcanic center of eruption during the period," beds of felstone, greenstone, ashes and conglomerates being there associated with those of the Old Red Sandstone and intersecting the latter in dikes.

In the *Lower Carboniferous*, occur the trap rocks of the Limerick basin, cotemporaneous with the toad-stones of Derbyshire, the trap and porphyrite ashes of the central valley of Scotland forming the Kilpatrick, Campsie and Dalry Hills. These Limerick trap rocks are of two times of eruption, the earlier being the most important. The trap is chloritic and contains chrysolite. Another district is that at the entrance of Bantry Bay, where the rocks are felstones, greenstones, beds of ash, breccia and conglomerate.

No volcanic ejections of the Permian or of the Mesozoic periods occur. The next in age and last are those of the Miocene Tertiary. They occur over the area of northeastern Ireland, and extend thence to the west of Scotland and the inner Hebrides. The area in Ireland is 2,200 to 2,300 square miles. These rocks are briefly described, from an article by Professor Hull, in the last volume of this Journal, at page 147.

7. *Asphaltic Coal from the shale of the Huron River, Ohio ;* by Prof. A. R. Leeds.—In the number of the Annals of the Lyceum of Natural History of New York for June, Professor Leeds describes a coaly substance from Ohio, resembling much the asphaltic coal or albertite of New Brunswick. The seam averages two inches; and it is traversed by innumerable sheets of sulphate of baryta.

Dr. J. S. Newberry adds a note to the paper stating that there are many localities of similar material in Ohio and Kentucky; and that it occurs in fissures of the Huron shale and was formed from distilled petroleum, like the albertite and grahamite. He observes that this shale, "which is the equivalent of the Portage group of the New York geologists," contains throughout from 10 to 25 per cent of carbonaceous matter, and is the source whence most of the oil is derived both in Pennsylvania and Ohio.

8. *Eruption of Tridimitic Ashes.*—Dr. A. Baltzer, of Zurich, has announced that the Island Volcano, one of the Lipari group, ejected, on the 7th of September, 1873, a whitish ashes for three hours, which formed a deposit over the interior of the island, in some parts three to four centimeters deep, which consisted of silica in the state called tridimite. He found the material to agree with that species in specific gravity, degree of solubility in alkali, action of polarized light and other respects. His announcement was made before the Society of Natural History of Zurich, on the 4th of January, 1875.—*R. Com. Geol. d'Italia*, 1875, 197.

9. *Exploration of the Colorado River of the West, and its Tributaries*, explored in 1869 to 1872 under the direction of the Secretary of the Smithsonian Institution; by Prof. J. W. Powell. 292 pages, 4to, with many plates, 8 maps.—The Smithsonian Institution has shown, by this volume of Professor Powell, that

Congress did well in placing this exploration of the Colorado under its direction. The volume has been, in part, foreshadowed by the magnificent photographs of the Colorado region, by the artist of the expedition, which have been widely distributed through the country. But the descriptions of Professor Powell, and the new birds,eye views and sections inserted alongside of the photographs which are reproduced in admirable style, have added ten-fold to the instruction from the latter. The work is to a large extent a treatise on denudation, or the making of valleys—and of mountain forms, too, by circumdenudation—and never was the subject more appropriately and grandly illustrated. The author divides the valleys of the region into two classes: 1st, the *Transverse*, running across the strike, and, 2d, the *Longitudinal*, running with the strike. Under each class he makes three kinds. Of *Transverse:* (*a*) those which pass through a fold, the *diaclinal;* (*b*) those running in the direction of the dip, the *cataclinal;* (*c*) those running against the dip, the *anaclinal.* Of *Longitudinal:* (A) Those which follow anticlinal axes, the *anticlinal;* (B) those which follow synclinal axes, the *synclinal;* and (C) those which follow the direction of the strike between the axes of the fold, the *monoclinal.* The subject of faults in the Colorado region is fully illustrated. Many readers of Professor Powell's article in vol. v, of this Journal (p. 456, 1873) were no doubt unsatisfied for the want of sections. These are here fully supplied.

The volume also gives much information on the Indians of the Colorado region. It closes with a chapter of 65 pages on the genera *Geomys* and *Thomomys* by Dr. Elliott Coues, U. S. A. There is also an Addendum on the Salamander of Florida (*Geomys tuza*), by Prof. G. Brown Goode, the direct connection of which with the subject of Professor Powell's Report is not apparent to us.

10. *Report of the Geological Survey of Ohio.* Vol. II, *Geology and Paleontology.* Part I, GEOLOGY. Officers of the Survey, J. S. NEWBERRY, Chief Geologist; E. B. ANDREWS and EDWARD ORTON, Assistant Geologists; F. G. WORMLEY, Chemist; F. B. MEEK, Paleontologist. 702 pp. 8vo, with plates, wood-cuts, and colored geological county maps. Columbus, Ohio. Published by authority of the Legislature of Ohio.—The survey, of which this volume is one of the series of Reports in course of publication, was carried forward during the years 1869 to 1874. The volume just issued is the second on the Geology of the State, although this might not be inferred from the ambiguous title-page. The first was issued two years since and is noticed in this Journal in vol. vi. The new volume commences with chapter xxx. The title-pages of the two volumes are intended to indicate—not "volume I, Geology and Paleontology, Part I, Geology," and "volume II, Geology and Paleontology, Part I, Geology," as they read—but Part I, Geology, vol. I, and Part. I, Geology, vol. II. A third volume, vol. III, will finish the Part I, Geology; and two volumes more will complete Part II, Paleontology, volume I having been published in 1873.

The Reports, as we have before said, are very valuable contributions to geological science, and as important to the economical interests of the State The paleontological volumes, if published with all the plates, according to the plan proposed, will redound to the honor of the State, and contribute to the progress of knowledge throughout it, if bringing no return to its treasury.

The present volume commences with a chapter on the Quaternary, or, as it is here called, Surface Geology, by Dr. Newberry —a chapter which has been noticed in the last volume of this Journal. Dr. Newberry has, next, valuable chapters on the Carboniferous System of Ohio and on the Geology of Erie and Lorain Counties. The succeeding chapters on Ottawa, Crawford, Morrow, Delaware, Van West, Union, Paulding, Hardin, Hancock, Wood, Putnam, Allen, Anglaize, Mercer, Henry and Defiance Counties are by N. H. Winchell; those on the Surface Geology of Southeastern Ohio, on Washington, Noble, Guernsey (southern half), Belmont (southern half), Monroe, Pickaway and Fairfield Counties, by E. B. Andrews, who has devoted much time to the coal formation; and those on Pike, Ross, and Greene Counties, concluding the volume, by Edward Orton.

The facts in these Reports have in part appeared in the Annual Reports published in the course of the survey, and have been the subjects of notices in this Journal. Without further discussion of their contents, we repeat that they are all worthy of thorough study. The geological maps of the counties, inserted in the volume, are well colored.

11. *Sixth Annual Report of the Geological Survey of Indiana made during the year* 1874; by E. T. Cox, State Geologist; assisted by Prof. JOHN COLLETT, Prof. W. W. BORDEN and Dr. G. M. LEVETTE. 288 pp. 8vo, with maps and plates.—This volume contains a Report by the State Geologist, Mr. Cox, on the Quaternary of Indiana, some of its economical products, its Indian antiquities, and the Geology of Jackson County, with a Geological map; a Report on the Geology of Brown County, with a map, by Prof. Collett; on the Geology of Scott and Jefferson Counties, with maps, by Prof. Borden; on the Sisco of Lake Tippecanoe and its relatives, by Prof. D. S. Jordan, M.D., with a wood-cut of *Argyrosomus sisco* Jordan; a synopsis of the genera of Fishes to be looked for in Illinois, by Prof. Jordan; and a partial list of the Flora of Jefferson Co., by J. M. Coulter.

Mr. Cox gives the following list of fossil shells found in the læss near New Harmony: *Macrocyclis concava* Say, *Zonites indentatus* Say, *Patula perspectiva* Say, *Helix lineata* Say, *H. labyrinthica* Say, *H. hirsuta* Say, *H. monodon* Rack., id. var. *fraterna*, *Punctum minutissimum* Lea, *Succinea elongata* Say, *Pupa armifera* Say, *Helicina occulta* Say, *Cyclostoma lapidaria* Say. The upper surface of the læss is 165 feet above the Wabash river in Posey Co., and in Perry Co., on the Ohio, about 200 feet.

The "Black shale," the formation which overlies the Corniferous limestone, has afforded Prof. Borden, at Lexington in Scott

Co., the following fossils—identified by R. P. Whitfield: *Leiorhynchus quadricostata* Hall, *Chonetes lepida* Hall, *Tentaculites fissurella* Hall, a fragment of a *Cardiola* near *C. radians*. The *Leiorhynchus* is a Genesee shale species; the *Tentaculites* is found as well in the Marcellus and Hamilton beds; the *Chonetes* belongs to the Hamilton and possibly also to the Genesee; while the *Cardiola* is a Devonian genus having species in the Upper Helderberg and Hamilton; which facts appear to establish the equivalency of the New Albany Black Shale and the Genesee shale; while the Goniatite shale, which rests upon it, represents the Kinderhook group (Subcarboniferous) of Illinois.

Kaolin, or white porcelain clay, occupying "pockets" in the Carboniferous rocks of Pope County, has been long known under the name of Golconda clay. In Lawrence County the clay constitutes a bed interstratified with the Carboniferous rocks, which is five to six feet thick (one-third pure white) and has a wide extent. It is underlaid by a bed of limonite five feet and less in thickness. It lies beneath the Millstone grit and seems to correspond to an upper member of the Archimedes limestone. The Kaolin is in part (A) Snow-white and pulverulent; in part (B) white to purplish-brown, and cutting like putty; and (C) in concretionary masses of a pea-green color fading in the light, with an unctuous feel. It is called *Indianaite* by Mr. Cox. The kaolin contains masses of allophane of a transparent emerald-green color, one specimen of which afforded Dr. J. Lawrence Smith: Silica 20 per cent, alumina 40, water 40. All degrees of gradation exist between the allophane and kaolin.

Analysis of the kaolin afforded—

	Si	Al	Fe	Mn	Ca, Mg	H
Var. A	45·90	40·34	—	—	—	13·26
Var. B	47·05	37·14	*tr.*	·03	·03	15·55
Var. C	47·13	36·76	*tr.*	—	·04	15·13
Golconda	42·28	43·05	—	—	*tr.*	14·66

The several County Reports with their maps present much valuable matter on the Quaternary, Devonian, Carboniferous and Subcarboniferous rocks. Prof. Collett states that the boulder clay of Northern Illinois has its southern limit in the northern part of Brown Co., with little thickness, but in Northern Illinois it becomes 150 to 250 feet thick; and he adds some facts respecting erosions by the waters from the melting glacier. Various sections illustrating the relations of the Black Shale to the other rocks are given by Professor Borden, whose discoveries in it are mentioned above.

12. *The Geological and Natural History Survey of Minnesota: The Third Annual Report*, for the year 1874; by N. H. Winchell, State Geologist. Submitted to the President of the University of Minnesota, Dec. 31, 1874.—Prof. Winchell, in this Report, treats of the Geology of Freeborn and Mower Counties. Of Freeborn County Mr. Winchell remarks, "There is not a natural exposure of the underlying rock" and "hence the details of its geological structure are wholly unknown." The field is

still not a barren one; for the surface is covered with the drift, and the material of which it consists affords some evidence of the rocks below; and, besides, a shaft has struck the Cretaceous in the northwestern portion of the county. It is inferred that the Cretaceous extends north and south across the west end of the county. The average thickness of the drift in Freeborn County is stated to be not far from 100 feet. A figure is given illustrating the flow-and-plunge structure of the sand and gravel at Albert-Lea. Clay for brick-making is obtained near this place about five feet below the surface. Clay, from the drift formation, is also found at Geneva.

Mower County also is buried in drift, but not to the concealing of all rock exposures. As elsewhere in the northwest, the drift-made soils are of the very best kind. The drift material is a "hard-pan clay" light in color for 10 to 15 feet, and below this usually bluish. It contains gravel and boulders, some of them of granite and very large. At Windom and LeRoy this drift overlies a peat bed.

The Lower Cretaceous underlies the western portion of the county; how far east it extends is unknown. The Cretaceous beds at Austin have afforded fossil leaves—one kind related to *Ficus primordialis* Hr., and another a *Sequoia*. Explorations for coal in these counties have obtained nothing that promises success. Lignite is found to encourage the search, but, Mr. Winchell observes, "after the tests that have already been made it can be pretty confidently stated that the lignites are at present of no economical value."

13. *Note on Lignite in the Cretaceous of Minnesota;* by N. H. Winchell. (From a letter to J. D. Dana, dated Minneapolis, Minn., Sept. 2, 1875).—An interesting discovery of lignite in the extreme northern portion of Minnesota has just been made. Explorations have been carried on during the summer by private parties in search of coal, and a hundred pounds of what was taken for coal were brought to Duluth from a point between Vermilion and Rainy lakes.

Similar exploration has revealed the lignites of the Cretaceous at a number of other points in Minnesota, extending from near the Iowa state line to the central portion of the state. Mr. Kloos has given an account of a "Cretaceous basin in the Sauk Valley" in this Journal, and Mr. Meek identifies it as the *Fort Benton* by the few fossils that were gathered. Inferentially the Cretaceous has been extended over the most of Minnesota, and even into the State of Michigan (First Annual Report of the Geological Survey of Minnesota, 1872), but this discovery not only shows that the Sauk Valley is probably not an isolated "basin," but also that the Cretaceous beds did extend, prior to the drift, at least, if they do not now, over the entire state from north to south. I have an account also of a probable Cretaceous outcrop in the state of Winconsin from Prof. Frank H. Bradley, but it has not been identified authentically.

14. *Note on the Age of the Cretaceous of Vancouver Island and Oregon;* by W. M. GABB. (From a letter to J. D. Dana, dated Philadelphia, Sept. 5, 1875.)—In the April Number of this Journal, p. 318, there is a note by Mr. Selwyn with reference to the age of the Nanaimo coal mines. While fully endorsing the opinion there expressed, it may be worth while to call attention to the fact that the same opinions were put on record in the Reports of the California Geological Survey, both by Prof. Whitney and myself, as much as eight years before the date of the volume of the Canadian Report quoted.

In this connection, I wish to note in the new edition of the "Manual of Geology," p. 456, top of page: The Cretaceous formation is said to occur "in Oregon, east of the Cascade Range (Marsh)," thereby giving to Prof. Marsh the credit of the first announcement.

Without wishing to disparage the excellent work done by that paleontologist, you have inadvertently done me an injustice, to which I call your attention. See Palæontology of California, vol. 2, p. 181, foot-note.

15. *Sub-Wealden Exploration.*—The Sub-Wealden boring has reached a depth of 1,672 feet. It was the opinion of British geologists that Paleozoic rocks would be found at a depth varying from 700 to 1,700 feet. So far, however, the strata are Mesozoic; but the latest fossils give some indications of a Paleozoic rock.—*Nature*, July 29.

16. *Das Elbthalgebirge in Sachsen.* I Theil, 8 Lief., and II Theil, 6 Lief. (Cassel, Th. Fischer.)—This great work, by Dr. GEINITZ, has been brought to a close by the publication of the two numbers here announced—the first (pp. 277 to 320) closing his descriptions of the fossils of the Lower Quader (Upper Green Sand of the Middle Cretaceous), and the second (pp. 199 to 246) of those of the Middle and Upper Quader (Upper Cretaceous). The lithographic illustrations, drawn partly by E. Fischer, and in part by Miss Elise Geinitz, daughter of the author, extend to 67 4to plates for the former and 46 for the latter, and are excellent; they include figures of species of Sponges (10½ plates) Foraminifera, Radiates (18½ plates), Bryozoans, Brachiopods, Conchifers, Gasteropods, Cephalopods, Worms, Crustaceans. Fishes, Reptiles and Plants. The remains of the Vertebrates of the Lower Quader, described and illustrated in Part I, include species of the genera of Sharks, *Oxyrhina*, *Otodus*, *Lamna*, *Scylliodus*, *Corax*, *Acrodus*, *Ptychodus;* of Ganoids, *Pycnodus;* and of Reptiles, of the genera, *Plesiosaurus* and *Leiodon*. The remains of Fishes, illustrated in Part II, include teeth of Sharks of the genera, *Oxyrhina*, *Lamna*, *Otodus*, *Notidanus*, *Corax*, *Ptychodus* and *Spinax;* remains of *Chimæra Agassizi* Buckl. and *C. Mantelli* Buckl.; under Ganoids, species of *Pycnodus*, *Hemicyclus*, etc.; under Ctenoids, *Beryx ornatus* Ag., *Hypsodon Lewesiensis* Ag., etc.; under Cycloids, *Cladocyclus*, *Strehlensis* Gein., *Saurocephalus lanciformis* Harlan, *S.? dispar* Hébert, *Enchodus halocyon* Ag., *Osmeroides Lewesiensis* Mant., *O. divaricatus* Gein., and *Cyclo-*

lepis Agassizi Gein.; and the Reptiles comprise the species *Plesiosaurus Bernardi* Owen, *Polyptychodon interruptus* Owen, *Chelonia Carusiana* Gein.

The work is full in its descriptions and synonymy, and many of the species have been first made known by Dr. Geinitz.

17. *Paleontological Cabinet of Prof. James Hall.*—Professor Hall's extensive collection of fossils has been purchased for the Central Park Museum, New York. The price reported to have been stipulated is $65,000.

18. *On two new varieties of Vermiculites;* by Prof. J. P. Cooke and Mr. F. A. Gooch. (Proc. Amer. Acad. Sci., of May 11, 1875).—In this paper, which may be regarded as supplementary to Prof. Cooke's elaborate memoir published early in 1874, the author describes two new varieties of vermiculites, one from Lerni, Delaware Co., Pa., and another from Pelham, Massachusetts. By means of careful experiments, the loss of water was ascertained after air-drying, and at 100° C., 300° C. and red heat. The analyses obtained and those previously made of other vermiculites, are discussed, and the following important conclusions reached.

1. That all the vermiculites are unisilicates.

2. That these minerals combine with water in several definite proportions—sustaining Prof. Cooke's former statement that the water in the vermiculites is water of crystallization.

3. That they may be reduced to the condition expressed by the atomic ratio (oxygen-ratio) for the silica, oxides and water, 2 : 2 : 1, which is therefore taken as the normal ratio.

4. That the only essential difference between the different varieties of vermiculites is in the ratio between the sesquioxide and protoxide bases.

The atomic ratios obtained for the air-dried, dried at 100°, and dried at 300°, specimens, are respectively, for

Hallite	4 : 4 : 3,	4 : 4 : 2½,	4 . 4 : 2
Pelhamite	4 : 4 : 4,	4 : 4 : 2,	4 : 4 : 1
Jefferisite	4 : 4 . 4,	4 : 4 : 2,	4 : 4 : 1

The Lerni mineral gave, dried at 100° C., the ratio 2 : 2 : 1 (=4 : 4 : 2); and the analysis afforded (mean of results)

$\dot{S}i$ 38·03, $\dot{A}l$ 12·93, $\dot{F}e$ 7·02, $\dot{F}e$ 0·50, $\dot{M}g$ 29·64, $\dot{H}$ 11·68=99·80.

19. *Dana's System of Mineralogy.*—A new "subedition" of this work—the sixth—has been brought out by the publishers, Messrs. Wiley & Son of New York; in other words, copies have been struck off from the plates *after making a number of corrections* as in the case of the other "subeditions." Moreover the copies of this subedition contain the two Appendixes to the work—the first by Professor Brush, and the second by Mr. E. S. Dana—adding thus 98 pages to the 828 of the original volume.

20. *Hematococcus lucustris, &c.*—Some account of researches upon this microscopic plant, and upon the foundations of a natural classification of the Chlorosporous Algæ, are just published by Rostafinski—lately a pupil of DeBary—in the Memoirs of the Academy of Sciences of Cherbourg (tom. xix, pp. 137–154, 8vo, 1875). The identity of *Protococcus, Hæmatococcus,* or *Chlamydi-*

coccus nivalis and *pluvialis* is made out; at least it is shown that the latter can live upon snow and ice, and that the development is identical. For the generic name of the *Red-snow* plant, &c. Agardh's name of *Hæmatococcus* is preferred, on good grounds: the specific name adopted is *lacustris*, Girod-Chantrans having well investigated the plant and figured and described it, under the name of *Volvox lacustris*, so long ago as the end of the last and the beginning of the present century (1797, 1802). *Hæmatococcus* propagates by two kinds of zoospores; i. e. sometimes by large and ordinary ones, resulting from the division of the contents of the cell or plant into four daughter-cells, each of which is transformed into a zoospore of somewhat complicated structure; while other individuals transform their contents into about 32 microzoospores. The development of both kinds of zoospores into the plant has been observed by Rostafinski. The development is non-sexual. Velter's supposed discovery of the copulation of the large zoospores is discredited and explained away. Rostafinski concludes that *Hæmatococcus* is devoid of sexual reproduction. Following up Decaisne's early hint that the reproductive organs of *Algæ* should furnish the characters for their natural arrangement, he indicates the principal groups or tribes of the *Chlorosporeæ* which have thus far been made out, by DeBary and others, with some re-organization. Thus, after the *Conjugatæ*, in which fecundation takes place by the conjunction of two immobile cells of the same value (i. e. with no distinction of male and female), he proposes to place a parallel tribe, *Isosporeæ*, in which there is a copulation of zoospores, the sex of which is equally indeterminate (*Hydrodictyon*, *Botrydium*, &c.). The third is *Oophoreæ* of DeBary (*Sphæroplea*, *Vaucheria*, *Œdogonium*, &c., to which Rostafinski adds *Volvox* and *Eudorina*); here the fecundation is by antheroroids and oospores. And he is disposed to take *Hæmatococcus* as the type of a fourth tribe, *Agameæ*, propagating non-sexually by spores. A. G.

21. *Catalogue of Plants growing without cultivation within thirty miles of Amherst College;* by Edward Tuckerman, M.A. and Charles Frost, M.A. Amherst, 1875. pp. 98, 8vo.—The Lower Cryptogamia, exclusive of the *Lichenes*, are by Mr. Frost. They fill half the Catalogue. Prof. Tuckerman contributes an interesting preface. The Catalogue bears marks of the scrupulous accuracy and neatness of finish which characterize all of Prof. Tuckerman's work, the simplest, no less than the more elaborate; and the arrangement, typography, and paper, are of corresponding excellence. We venture to correct a correction. We are told at the close to "read *Calystegia Sepium*"; but we prefer to read as it stands in its place "*Calystegia sepium.*" Such substantive genitive plurals, not being proper names, surely need no capital initial, nor are they accustomed to have them. A few plants have been detected in the district which would not have been expected and are not before recorded. *Blephilia ciliata* is the most noteworthy of these. We are curious to know how many species this model catalogue contains, and count it an oversight that the number is

not reckoned. A rough and rapid enumeration gives 1066 Phænogamous species, 59 Acrogenous Cryptogamia, 216 Anophytes, 1361 Thallophytes, of which over eleven hundred are Fungi; so that the Cryptogamia amount to 1839.

We may hope that the time is not far distant when we may have Manuals of all our Lower Cryptogamia, beginning with the *Lichenes* by Prof. Tuckerman. A. G.

22. *Serjania Sapindacearum Genus monographice descriptum. Monographie der Sapindaceen-Gattung Serjania*, Von L. Radlkofer. Munich, 1875. pp. 392, 4to. Published by the Royal Bavarian Academy.—To this memoir the quinquennial prize founded by the will of the elder DeCandolle has been awarded, and it well deserves it. It is one of the most elaborate, painstaking, and apparently thorough pieces of monographical work in systematic botany that we have ever seen. Undertaken as it is by one who has proved, at the commencement of his career, his ability for microscopical investigation (when Radlkofer gave the *quietus* to the theory of the origination of the embryo from the pollen-tube), we may hope that an investigation of this kind will not be detrimental to the author's prospects, in a country where it has been the fashion to undervalue everything not microscopical, and even that it may betoken a rising appreciation of systematic botany in Germany. The subject is too technical to call for a detailed review in this Journal. We merely note that 145 species of Serjania are characterized, described, and their synonymy completely exhibited. Nearly all extant materials appear to have been under the author's hands. A. G.

23. *Zur Keimungsgeschichte der Charen, von* A. de Bary.—In the present paper, extracted from the Botanische Zeitung, Prof. De Bary gives a detailed account of the manner in which the prothallus is produced from the spore in the Charæ. That the new *Chara*-plant does not spring directly from the spore was first shown by Pringsheim, who noticed that the plant is a lateral outgrowth from an intermediary filamentous structure, the *vorkeim* (prothallus). De Bary finds that a lenticular portion of the spore projects beyond the mass of the spore from which it is soon separated by a wall. The lenticular portion is then divided into portions, one of which develops into the prothallus proper, while the other becomes what is known as the primary root in *Chara*, although it does not correspond to the structure of the same name in phanerogams. In passing, reference is made to parthenogenesis in *Chara crinita*, which fact is confirmed by De Bary, who finds that female plants isolated in closed glass vessels fruit abundantly. W. G. F.

24. *Description of a new Crustacean from the Water-lime group at Buffalo;* by Aug. R. Grote and W. H. Pitt, (Bulletin of the Buffalo Society of Natural Science, vol. iii, July, 1875.)—The specimen shown exhibits an impression of the ventral surface of a new form of Crustacean allied to *Eurypterus Pterygotus*, for which the name *Eusarcus scorpionis* is proposed. The cephalothoracic portion appears to be separate from the body,

and to be considerably narrower in proportion than in allied forms. The legs are in the same number as in *Eurypterus*. The swimming feet appear to differ by the straighter, less rounded outer margins. In the specimen the rhomboidal plates are not given. From the impressions of the joints of the swimming feet their relative dimension does not seem to accord with *Eurypterus*. The four pair of anterior feet proceed from two elongate oral plates of which the impression is very distinct. The spines of the anterior feet appear to be long, curved, and to have an anterior direction. The absence of chelate appendages to the posterior margin of the feet is particularly noticeable. The first seven broad segments of the abdomen form a large ellipse. There is an evident and remarkable narrowing of the succeeding caudal segments. Of these six appear to be made out on the specimen. The surface of the cast is punctate with scattered triangular impressions. The cast shows a widening of the terminal segment and no traces of a spiniform process are exhibited. This portion of the fossil was imbedded in the matrix, and seems to have been bent downward and outward in the specimen. The greater flexibility of the caudal segments may be inferred from their shape and position. The specimen from which the above description is drawn was found in the Water-lime group at Buffalo, N. Y., in the same bed which has furnished specimens of *Eurypterus lacustris*. The length of the entire specimen is two hundred and fifty millimeters; the greatest width of the body is one hundred and ten millimeters.

"The interest which attaches to this remarkable Crustacean arises from the discovery of a form which may be allowed to be higher than *Eurypterus* and *Pterygotus* from the peculiar differentiation of the body expressed by the narrowness of its cephalothoracic portion, and the sudden constriction of the terminal segments." The article is accompanied by a fine plate.

25. *Bathybius.*—Prof. Huxley in connection with the publication of a letter to him from Prof. Wyville Thomson in *Nature* of August 19th, remarks as follows:—Prof. Wyville Thomson further informs me that the best efforts of the *Challenger's* staff have failed to discover *Bathybius* in a fresh state, and that it is seriously suspected that the thing to which I gave the name is little more than sulphate of lime, precipitated in a flocculent state from the seawater by the strong alcohol in which the specimens of the deep-sea soundings which I examined were preserved.

"The strange thing is, that this inorganic precipitate is scarcely to be distinguished from precipitated albumen, and it resembles, perhaps even more closely, the proligerous pellicle on the surface of a putrescent infusion (except in the absence of all moving particles), coloring irregularly but very fully with carmine, running into patches with defined edges, and in every way comporting itself like an organic thing."

Prof. Thomson speaks very guardedly, and does not consider the fate of *Bathybius* to be as yet absolutely decided. But since I am mainly responsible for the mistake, if it be one, of introduc-

ing this singular substance into the list of living things, I think I shall err on the right side in attaching even greater weight than he does to the view which he suggests.—*Nature.*

III. Miscellaneous Scientific Intelligence.

1. *American Association.*—The meeting of the Association at Detroit was well attended, and, as the list below shows, many papers of interest were presented. The address of the retiring President, Dr. John L. LeConte, is republished in pages 241–256. An excellent address was delivered by Prof. H. A. Newton, Vice President of the Physical section, on the importance of the Study of Mathematics for the student in all branches of science, and another, by Dr. J. W. Dawson, before the Geological and Natural History section, presenting some of the difficulties which geology appears to place in the way of Darwinism. The Chemical section, organized at the preceding meeting, at Hartford, brought together many chemists.

The next meeting of the Association is to be held at Buffalo. The following officers were appointed for that meeting: President, Prof. Wm. B. Rogers; General Secretary, F. C. Mendenhall; Vice President of Section A, Charles A. Young, of Hanover, N. H.; of Section B, Edward S. Morse, of Salem, Mass. F. W. Putnam, of Salem, Mass., is Permanent Secretary.

The following is a list of the papers read either in full or by abstract.

I. *Mathematics, Physics, Astronomy.*

The Solar Atmosphere; S. P. Langley.

Account of the Measurement of a Primary Base Line in the U. S. Coast Survey, near Atlanta, Ga.; J. E. Hilgard.

Problems in Watson's Coordinates; T. Hill.

Distribution of the Asteroids; D. Kirkwood.

Descriptive Geometry; S. E. Warren.

An Acoustic Method of Measuring the Velocity of Electricty; J. Lovering.

A Systematic Method for indicating the Localities of a County, and rapidly discovering Places in a Map; J. M. Toner.

Tides of New York Harbor; W. Ferrel.

Algebraic curves expressed in trigonometric equations; H. A. Newton.

Transformation of curves from Algebraic to Transcendental; H. A. Newton.

Geometrical representation of certain Diophantine Problems; H. A. Newton.

Path of Iowa County Meteorite, with exhibition of fragments of the same; N. R. Leonard.

A new Meteorological Instrument; J. W. Osborne.

The Relative Intensity of the broken lines of Metallic Spectra; G. F. Barker.

A New Vertical Lantern Galvanometer; G. F. Barker.

On some Inequalities of Long Period in the Moon's Motion; J. N. Stockwell.

Adjustment of the Gregorian Calendar; E. B. Elliott.

The Subsidiary Principle as applied to Coinage and Money account; E. B. Elliott.

Description of an apparatus for the automatic determination of Atmospheric resistance; G. W. Hough.

Method of exhibiting by Projection the difference in rapidity of transmission of Sound through various Gases; T. C. Mendenhall.

Temperature of two Deep Borings in Indiana; E. T. Cox.

II. *Chemistry.*

On an improvement of Bunsen's method for specific gravities of gases, etc.; T. C. MENDENHALL.

Chemistry of three dimensions; F. W. CLARKE.

On certain new Tungsten compounds; A. R. LEEDS.

On the omission of Dynamical Quantities from Chemical Formulæ; H. F. WALLING.

Carbon determinations in Iron and Steel; J. W. LANGLEY.

Comparative determinations of the solubilities of Alkaloids in Crystalline, Amorphous, and Nascent conditions; A. B. PRESCOTT.

Note on Colorado Tellurium; F. W. CLARKE.

On the requisite amount of simple friction of soft iron against cold steel to melt it; B. S. HEDRICK.

On Otto's method of estimating Phosphoric Acid in presence of Iron and Aluminium; S. W. JOHNSON.

Apparatus for fat extraction; S. W. JOHNSON.

Composition of corn fodder and yield per acre; S. W. JOHNSON.

Composition of the Sweet Potato; S. W. JOHNSON.

On Thorpe's method of estimating Nitric Acid; S. W. JOHNSON.

A Convenient Instrument for showing the Absorption of Hydrogen Gas by Palladium; J. L. SMITH.

On Graphitic Oxide as prepared from the Graphites of the Sevier Co. and Dekalb Co. Meteorites; J. L. SMITH.

Notice of an undescribed Meteorite that fell in Wisconsin in 1865; J. L. SMITH.

Exhibition of a large quantity of Cæsium Alum, with some remarks on its Preparation; J. L. SMITH.

On certain precipitates containing Chloride of Silver; F. W. CLARKE.

III. *Geology and Paleontology.*

Geology of the Southern Counties of New York, and particularly of the Catskill Mountain Region; J. HALL.

A comparison between the Northern (or Ohio) and the Southern (or W. Va.) side of the Alleghany Coal Field; E. B. ANDREWS.

New and remarkable Coal-plants from the lower Coal Measures of Ohio, and their typical relations; E. B. ANDREWS.

New specimens of Fossils from Canada; J. W. DAWSON.

Physical Geology of the trap formation of Lake Superior; C. WHITTLESEY.

Physical Geology of the Ohio coal-field; C. WHITTLESEY.

On Ancient Glaciation at Kelley's Island, Ohio; C. WHITTLESEY.

Ancient Peat in the Drift deposits of Minnesota; N. H. WINCHELL.

Rectification of the Geological Map of Michigan; A. WINCHELL.

Evidence of Glacial Action upon the summit of Mt. Washington, N. H.; C. H. HITCHCOCK.

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On some new Fossil Fishes and their Zoological Relations; J. S. NEWBERRY.

On some of the Results of the Geological Survey of Ohio; J. S. NEWBERRY.

The Ice Period in the Mississippi Valley; its phenomena and causes; J. S. NEWBERRY.

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New and Interesting Insects from the Carboniferous of Cape Breton; S. H. SCUDDER.

On the supposed Ancient Outlet of Great Salt Lake; A. S. PACKARD, Jr.

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Remarks on the Hot Springs and Geysers, and other topics illustrating the scientific value of the Yellowstone Park; T. B. COMSTOCK.

Exhibition of some Curious disks of Silica in the structure of Coal from Tennessee; J. L. SMITH.

IV. *Zoology and Botany.*

The effect of the Glacial Epoch upon the Distribution of Insects in North America; A. R. GROTE.

Are Potato Bugs poisonous? A. R. GROTE and A. KAYSER.

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The Locust Plague in America: how to avert it; C. V. RILEY.

On the method of subduing Insects Injurious to Agriculture; J. L. LECONTE.

Symmetry and want of Symmetry of Leaves; W. J. BEAL.

Venation of Leaves of Dragon-Root and Martynia compared; W. J. BEAL.

Additional Observations on the Tarsus and Carpus of Birds; E. S. MORSE.

Distribution of Batrachia and Reptilia of North America; E. D. COPE.

Indications of Descent exhibited by North American Tertiary Mammalia; E. D. COPE.

Protozoan Studies; W. S. BARNARD.

On leading divisions in Recent and Fossil Chitonidæ; P. P. CARPENTER.

A method of Bleaching Wings of Lepidoptera to facilitate the study of their venation; G. DIMMOCK.

Observations on the Development of *Didelphys Virginiana;* W. S. BARNARD.

Membral Myology of *Simia Satyrus* as compared with Man and the higher Apes; W. S. BARNARD.

Demonstration of Locomotion in the Larvæ of Oestridæ; O. S. WESTCOTT.

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Some Observations on the Structure and Habits of *Utricularia vulgaris* (Carnivorous? plant); T. B. COMSTOCK.

On hybernation as exhibited in the Striped Gopher; P. R. HOY.

Gestation of Bats; B. G. WILDER.

Affinities and Ancestry of the Existing Sirenia; B. G. WILDER.

Notes on Menobranchus and Menopoma; B. G. WILDER.

Notes on the American Ganoids (*Amia, Lepidosteus, Acipenser* and *Polyodon*; B. G. WILDER.

Structure and Economy of Lamprey Eels (*Petromyzon*); B. G. WILDER.

The rectal pouch of Sharks and Skates, as perhaps representing the allantois of Air-breathing Vertebrates; B. G. WILDER.

V. *Ethnography and Archæology.*

Fac similes from Nature of Ancient Rock Sculptures in Ohio, with verbal explanations; C. WHITTLESEY.

Recent Mound Explorations at Davenport, Iowa; R. J. FARQUHARSON.

Indian Mounds and Shell-heaps near Pensacola, Florida; G. M. STERNBERG.

Notes on Aboriginal Money of California; L. G. YATES.

The Ancient Men of the Great Lakes; H. GILLMAN.

Ancient Earthworks near Anderson, Ind.; E. T. COX.

Rates of Human Progress; L. H. MORGAN.

Arts of Subsistence; L. H. MORGAN.

Ethnical Periods; L. H. MORGAN.

Mound Explorations in Kent County, Michigan; E. A. STRONG.

Archæological Notes from Wyoming; T. B. COMSTOCK.

2. *Foreign Scientific Associations.*—The meeting of the British Association at Bristol opened on Wednesday, the 25th of August. *Nature*, in the number for the 26th, and several following numbers, contains an extended report of the Proceedings.

The French Association began its session at Nantes on the 19th of August; and the Italian, at Palermo, on the 29th.

3. *The British Scientific Expedition.*—A third Report of the proceedings of H. M. S. Challenger has been issued by the Ad-

miralty, giving an outline of the work done, in connection with oceanic geography and its physical condition, between March and November of last year.

Having arrived in safety at Melbourne from her Antarctic cruise, the Challenger left Port Philip on the 1st of April for Sydney, at which place the ship was docked, refitted and completed with coal and provisions. During the stay in that harbor, Prof. Wyville Thomson made a journey to Queensland for the purpose of studying the natural history of tropical Australia.

On the 12th of June the ship left Port Jackson, on her voyage to New Zealand meeting with stormy weather, so stormy indeed as greatly to impede the work of sounding across, and it was unfortunate, as much interest was felt both in New South Wales and New Zealand in regard to the adaptability of the channel bed between them for telegraphic purposes. From the Australian shore the water deepened gradually until a depth was attained of 2600 fathoms, about one-third the distance across; the bottom at this depth consisted of the usual red clay, found in the deepest soundings. A depth of 275 fathoms, with rocky bottom, was found about 280 miles from New Zealand, with slightly deeper water within it. On the 25th, Port Hardy was reached, and Port Nichelson on the 27th. A seaman was unfortunately washed overboard and drowned in Cook Strait. The weather continued wild during the Challenger's stay at Wellington. On the 6th of of July she proceeded on her voyage, passing northward along the east coast of the north island, and on toward the Kermedec group; the stormy weather experienced precluded sounding operations, and the depth between New Zealand and those islands has still to be decided. The results of the dredging near Kermedec proved that animal life was much the same as at the same depths off the coast of Portugal, but several species new to science were obtained.

On the 17th of July, in lat. 25° 5′ S. and long. 172° 56′ E., the deepest cast since quitting the Atlantic was obtained—2,900 fathoms, the bottom temperature being 32°·9, by which temperature it is conclusive that there is a continuous and deep channel extending from the southward into these seas. The Expedition remained two days at Tongatábu, and then left for the Fiji Islands. The ship left the Fiji islands on the 10th of August, and when about thirty miles from the land, a depth was sounded of 1350 fathoms and the red ooze found, which had previously been obtained at only much greater depths. On the 15th, when about midway between Fiji and the New Hebrides group, a cast was had of 1450 fathoms, with the same nature of bottom.

Passing through between the islands of the last-named group, the Challenger proceeded on her way toward Raine Island. A sounding of 2650 fathoms was had, and then four others, slightly shoaling. The serial temperatures of these soundings proved one physical fact, viz: that the sea was cut off by a surrounding ridge, over which the greatest depth of water of any channel through it is 1,300 fathoms, the temperature at that depth being 35°, and then

continuing the same to the bottom. Below the 1,300 fathoms in the hollow between New Hebrides and Torres Strait the water is comparatively stagnant, as in the Mediterranean and other cut-off seas.

When 170 miles from Raine Island, the depth was 1,700 fathoms, and at seventy-four miles 1400 fathoms, showing that the inclination of the bottom of the sea was gradual up to the barrier reef. After anchoring for one night near Raine Island, the Expedition reached Somerset, Cape York, on the 1st of September, where they remained a week, and then proceeded through the Banda Sea, carrying a continuous line of soundings, and touching at Dobbo, Ki Doulan, and Banda. They arrived at Amboina on the 6th of October, where they obtained coal, and then, passing through the Molucca passage, reached Ilo Ilo on the 28th, Manila on the 4th of November, and Hong Kong on the 19th.

From the temperatures obtained in the Banda, Celebes, Sula, and China Seas, it is evident that their waters are cut off from the general oceanic circulation by ridges conecting the islands which surround them.

In each of the seas soundings of over 2,000 fathoms were obtained, but in no instance did the temperature decrease regularly from the surface to the bottom, as is usual in the ocean. In every case, after attaining a certain depth, the temperature below that depth remained the same; thus, in the Banda and China seas, the temperature remained the same from 900 fathoms to the bottom; in the Celebes Sea from 700 fathoms to the bottom; and in the Sula Sea from 400 fathoms to the bottom.—*Athenæum*, Aug. 7.

3. *Images produced by Lightning.*—A letter of Professor W. L. Brown, of the University of Georgia, to one of the editors, calls attention to a remarkable instance of the formation of impressions upon the human body by a lightning stroke, and encloses letters of Messrs. E. G. Simmons and T. N. Hawkes, of Americus, Ga., where the occurrence took place, describing the circumstances and some of the details of the phenomenon.

On the 12th of July, 1875, at about 4 P. M., as Mr. Simmons states, a stroke of lightning fell upon a house in Americus, rendering insensible for a time four persons who were seated in one of the rooms. The two outer sides of this room, which was at a corner of the house, had each one window, and nearly opposite these on the two other sides were the chimney and the door respectively. Outside, a tree stood in front of each of the windows, and about twelve feet from the house. A third tree, a locust, stood opposite the outer corner of the room and about the same distance from the house as the others. This tree was severed by the lightning, but the other two were not affected. A young child was sitting near the center of the room, while the mother and a young lady were seated not far from the chimney, near which, and close to the wall, was another child.

All these persons were rendered insensible for a time by the stroke which severed the tree, and on their recovery there were found impressed upon the bodies of them all more or less distinct

images of this tree. It was most distinct in the case of the child near the center of the room. Mr. Simmons says, "the child is impressed upon its back and exactly opposite upon its stomach. The entire tree is plain, and perfect in *toto;* every limb, branch, and leaf, and even the severed part, is plainly perceptible. It is a perfect photograph of the tree, and could not have been made more perfect by a painter's brush. It impressed the young lady upon the left hip and right leg," the mark being quite as perfect as that upon the body of the child. The mother and other child also bore less distinct impressions upon the leg. Mr. Hawkes says, "I saw the youngest child and the marks upon it. The marks on the lower part of the spine are exactly of the shape of a branch of the locust tree. Where the leaves are, on the branch, the mark does not take the shape of the entire leaf, but only of the skeleton of a leaf. The mark was about twelve inches long, and had a fork, exactly like the locust limb. It looked as though the blood was drawn to the skin along the marked part." The marks were not permament, as Prof. Brown, in his letter dated Aug. 7, adds, "a recent letter informs me that the impressions are no longer distinct."

Although many similar cases have been reported, the phenomenon described is rare enough to make a well-marked instance like this worthy of record. The formation of the images in all such cases is readily explained by a peculiarity in the mode of the electric discharge under certain conditions, by imitating which it is possible to produce similar figures artificially with an electrical machine, such as the Holtz machine, capable of giving electricity of very high tension. When the poles of the latter are strongly charged and are separated to the distance of a few inches, the discharge, instead of producing a spark or brush, sometimes consists of a very small jet upon the negative, and a sort of phosphorescent glow upon the positive. The space between them, though not luminous, is the seat of a discharging action which appears to take place along definite lines, like a stream or current, and is sometimes called the dark discharge. An object placed between the poles, and in the path of the discharge, interrupts this, and destroys the glow upon the positive pole in points corresponding to the lines thus broken; and in this way there is produced an image or shadow of the interposed object, which is often strikingly distinct and perfect.

In the case above described the phenomena are readily accounted for, if we suppose the thunder-cloud to have been negatively charged, and the tree to have stood in the path of the dark discharge which preceded or accompanied the lightning stroke, the action having been sufficiently intense, and the quantity of electricity great enough, to produce a visible impression upon the delicate tissues of the skin.

A more particular account of the subject may be found in two articles published in this Journal, May, 1870, page 381, and June, 1871, page 437. A. W. W.

5. *Annual Report of the Argentine Meteorological Office*, for the year 1874. 18 pp. 8vo. Buenos Aires, 1875.—This Report, by the Director of the office, Prof. B. A. Gould, gives details as to the arrangements for making meteorological observations which have been introduced into the Argentine Republic, and the present needs in order that the objects of the meteorological office may be efficiently carried out and rendered of permanent value to science. Tables of the mean temperature and barometric pressure, of principal points, made out from the observations thus far obtained, are given. Mr. Gould states that while at Cordoba the humidity of the air has its maximum in summer, the amount of moisture in February averaging 80 per cent of that requisite for complete saturation of the atmosphere, and its minimum, 50 per cent in September, at Buenos Aires, on the contrary, the maximum is usually in June, 83 per cent, and the minimum in December, 60 per cent. During the summer of 1874 the temperature of the solar rays was found at Cordoba to be on December 7th not less than 74·5° C., and it reached 77·8° on the following January 8th. The minimum under a clear sky, 52·5° was observed on the 19th of June.

Mr. Gould is making his comprehensive and thorough knowledge of terrestrial physics as well as astronomy tell in various ways on the progress of science in South America.

6. *Appletons' American Cyclopædia*, Vol. XI and XII, 1875. —The opening article of volume XI, Magnetism, is by Prof. Joseph Henry. It discusses magnetism and diamagnetism, terrestrial magnetism, magneto-electricity, induced currents from discharges of ordinary electricity, and induction in masses of metal in motion. Prof. Cleveland Abbe of the U. S. Signal Service contributes the article Meteorology, which is illustrated by numerous diagrams many his own or from original American sources. It is an admirable digest of the present state of this science. Molecule, by Prof. J. P. Cooke of Cambridge, is an article worthy of its author, setting forth the latest physico-chemical deductions, and the author's own views, in a clear and concise manner. The articles Metal and Meteorites, Ore dressing, and extraction of metals from their ores by chemical means, are furnished by Dr. Thos. M. Drown of Easton and Dr. R. W. Raymond; Mineral deposits, by Dr. J. S. Newberry; Microscope, by Prof. A. M. Mayer; Mediterranean Sea, by Count Pourtales; Meteor, Mars, Mercury, Moon, by Richard A. Proctor, London; and the metal Mercury, and Mine, by Dr. Raymond.

In volume XII, some of the leading scientific articles are—Paleontology, by Prof. James Hall; Mountain, by Dr. T. S. Hunt; the Theory of Music, by Prof. Mayer; Nebular hypothesis and other astronomical articles, by R. A. Proctor; Chemical Nomenclature, by Prof. Frank H. Storer of Harvard College. The botanical articles are by Prof. Geo. Thurber; those on Zoology by Dr. S. Kneeland of Boston; on Materia Medica by Dr. E. H. Clark of Harvard University; the medical and physiological by Dr. John C. Dalton; while Drs. Joy and Hogeboom furnish

various chemical articles. From this list of well-known authors and investigators, it is evident that the scientific character of the new edition of Appletons' Cyclopedia is worthy, as a whole, of commendation. We miss occasionally a well-known name among the biographical sketches; for example, Dr. Charles G. Page, whose contributions to dynamical electricity are second only, in this country, to those of Prof. Henry; and the well-known paleontologist F. B. Meek. Other like omissions could easily be mentioned, while some names have a prominence greater than history will award them. B. S.

7. *Boston Society of Natural History.*—The Paleontological collections of the Boston Society of Natural History have been recently much enlarged by the purchase of the collection of Herr Eser of Stuttgart, which is particularly rich in fossils of the Tertiary and Mesozoic formations, including the Triassic. Its specimens of Tertiary plants are of such delicacy that they are mounted, like botanical specimens, on paper. It was secured for the society by the efforts of Mr. A. Hyatt, during his visit in 1874 to Europe, and through the generosity of Mr. John Cummings. In addition several specimens of Ichthyosaurs and Teleosaurs were procured.

8. *Geographical Congress at Paris.*—The recent Geographical Congress at Paris awarded *Letters of Distinction* (highest award) to the Secretary of the Navy for the work of the Coast Survey: and to General Myer, of the Signal Service Bureau; *Medals of the First Class* to Francis A. Walker, for his Physical Atlas of the United States; Dr. F. V. Hayden, for his Rocky Mountain Geological Exploration; and to the Bureau of Statistics, of the Interior Department;—*Medals of the Second Class* to the Committee of Emigration, and the Members of the Tuscarora Expedition.

OBITUARY.

Dr. I. A. Lapham, of Milwaukee, Wisconsin, lately at the head of the Geological Survey of that State, died on the 14th of September. Dr. Lapham was by profession a surveyor, but he was also a good worker in geology, meteorology, and American archæology. Several years since he published a geological map of Wisconsin, and also a map illustrating the influence of the Great Lakes on the climate of Wisconsin. He early drew attention to the Indian mounds of Wisconsin, and a work by him, illustrated by numerous plates, entitled "Antiquities of Wisconsin," was published, in 1855, in the Smithsonian Contributions to Knowledge.

Carl Johann August Theodor Scheerer, the mineralogist, metallurgist, and chemist, died at Freiberg, on the 20th of July last, having been born in Berlin on the 28th of August, 1813. He was the author of various works and memoirs of high merit; among the former his *Lehrbuch der Eisenhüttenkunde* and his well known *Löthrhorbuch* (a treatise on the blowpipe), and among the latter a paper on Paramorphism in its relations to chemistry, mineralogy, and geology.